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THE

ENGLISH WORKS

OF

THOMAS HOBBES

OF MALMESBURY;

NOW FIRST COLLECTED AND EDITED

BY

SIR WILLIAM MOLESWORTH, BART.

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SEVEN
PHILOSOPHICAL PROBLEMS
AND
TWO PROPOSITIONS OF GEOMETRY.

BY
THOMAS HOBBES
OF MALMESBURY.

WITH
AN APOLOGY FOR HIMSELF AND HIS WRITINGS.

DEDICATED TO THE KING IN THE YEAR 1662.

TO THE KING.

THAT which I do here most humbly present to your sacred Majesty, is the best part of my meditations upon the natural causes of events, both of such as are commonly known, and of such as have been of late artificially exhibited by the curious.

They are ranged under seven heads. 1. Problems of gravity. 2. Problems of tides. 3. Problems of vacuum. 4. Problems of heat. 5. Problems of hard and soft. 6. Problems of wind and weather. 7. Problems of motion perpendicular and oblique, &c. To which I have added two propositions of Geometry: one is, the duplication of the cube, hitherto sought in vain; the other, a detection of the absurd use of arithmetic, as it is now applied to geometry.

The doctrine of natural causes hath not infallible and evident principles. For there is no effect which the power of God cannot produce by many several ways.

But seeing all effects are produced by motion, he that supposing some one or more motions, can derive from them the necessity of that effect whose cause is required, has done all that is to be expected from natural reason. And though he prove not that the thing was thus produced, yet he proves that thus it may be produced when the materials and the power of moving are in our hands: which is as useful as if the causes themselves were known. And notwithstanding the absence of rigorous demonstration, this contemplation of nature (if not rendered obscure by empty terms) is the most noble employment of the mind that can be, to such as are at leisure from their necessary business.

This that I have done I know is an unworthy present to be offered to a king: though considered, as God considers offerings, together with the mind and fortune of the offerer, I hope will not be to your Majesty unacceptable.

But that which I chiefly consider in it is, that my writing should be tried by your Majesty's excellent reason, untainted with the language that has been invented or made use of by men when they were puzzled; and who is acquainted with all the experiments of the time; and whose approbation, if I have the good fortune to obtain it, will protect my reasoning from the contempt of my adversaries.

I will not break the custom of joining to my offering a prayer; and it is, that your Majesty will be pleased to pardon this following short apology for my *Leviathan*. Not that I rely upon apologies, but upon your Majesty's most gracious general pardon.

That which is in it of theology, contrary to the general current of divines, is not put there as my opinion, but propounded with submission to those that have the power ecclesiastical.

I did never after, either in writing or discourse, maintain it.

There is nothing in it against episcopacy; I cannot therefore imagine what reason any episcopal man can have to speak of me, as I hear some of them do, as of an atheist, or man of no religion, unless it be for making the authority of the Church wholly upon the regal power; which I hope your Majesty will think is neither atheism nor heresy.

But what had I to do to meddle with matters of that nature, seeing religion is not philosophy, but law?

It was written in a time when the pretence of Christ's kingdom was made use of for the most horrid actions that can be imagined; and it was in just indignation of that, that I desired to see the bottom of that doctrine of the kingdom of Christ, which divers ministers then preached for a pretence to their rebellion: which may reasonably extenuate, though not excuse the writing of it.

There is therefore no ground for so great a calumny in my writing. There is no sign of it in my life; and for my religion, when I was at the point of death at St. Germain's, the Bishop of Durham can bear witness of it, if he be asked. Therefore I most humbly beseech your sacred Majesty not to believe so ill of me upon reports, that proceed often, and may do so now, from the displeasure which commonly ariseth from difference in opinion; nor to think the worse of me, if snatching up all the weapons to fight against your enemies, I lighted upon one that had a double edge.

Your Majesty's poor and

most loyal subject,

THOMAS HOBBES.

PHILOSOPHICAL PROBLEMS.

CHAPTER I.

PROBLEMS OF GRAVITY.

CHAP. I. Problems of gravity.

A. WHAT may be the cause, think you, that stones and other bodies thrown upward, or carried up and left to their liberty, fall down again, for aught a man can see, of their own accord? I do not think with the old philosophers, that they have any love to the earth; or are sullen, that they will neither go nor stay. And yet I cannot imagine, what body there is above that should drive them back.

B. For my part, I believe the cause of their descending is not in any natural appetite of the bodies that descend; but rather that the globe of the earth hath some special motion, by which it more easily casteth off the air than it doth other bodies. And then this descent of those we call heavy bodies must of necessity follow, unless there be some empty spaces in the world to receive them. For when the air is thrown off from the earth, somewhat must come into the place of it, in case the world be full: and it must be those things which are hardliest cast off, that is, those things which we say are heavy.

A. But suppose there be no place empty, (for

Problems of gravity.

I will defer the question till anon), how can the earth cast off either the air or anything else?

B. I shall show you how, and that by a familiar example. If you lay both your hands upon a basin with water in it, how little soever, and move it circularly, and continue that motion for a while; and you shall see the water rise upon the sides, and fly over. By which you may be assured that there is a kind of circulating motion, which would cast off such bodies as are contiguous to the body so moved.

A. I know very well there is; and it is the same motion which country people use to purge their corn; for the chaff and straws, by casting the grain to the sides of the sieve, will come towards the middle. But I would see the figure.

B. Here it is. There is a circle pricked out, whose centre is A, and three less circles, whose centres are B, C, D. Let every one of them represent the earth, as it goeth from B to C, and from C to D, always touching the uttermost circle and throwing off the air, as is marked at E and F. And if the world were not full, there would follow by this scattering of the air, a great deal of space left empty. But supposing the world full, there must be a perpetual shifting of the air, one part into the place of another.

A. But what makes a stone come down, suppose from G?

B. If the air be thrown up beyond G, it will follow that at the last, if the motion be continued, all the air will be above G, that is, above the stone; which cannot be, till the stone be at the earth.

A. But why comes it down still with increasing wiftness?

Problems of gravity.

B. Because as it descends and is already in motion, it receiveth a new impression from the same cause, which is the air, whereof as part mounteth, part also must descend, supposing as we have done the plentitude of the world. For, as you may observe by the figure, the motion of the earth, according to the diameter of the uttermost circle, is progressive; and so the whole motion is compounded of two motions, one circular and the other progressive; and consequently the air ascends and circulates at once. And because the stone descending receiveth a new pressure in every point of its way, the motion thereof must needs be accelerated.

A. It is true; for it will be accelerated equally in equal times; and the way it makes will increase in a double proportion to the times, as hath heretofore been demonstrated by Galileo. I see the solution now of an experiment, which before did not a little puzzle me. You know that if two plummets hang by two strings of equal length, and you remove them from the perpendicular equally, I mean in equal angles, and then let them go, they will make their turns and returns together and in equal times; and though the arches they describe grow continually less and less, yet the times they spend in the greater arches will still be equal to the time they spend in the lesser.

B. It is true. Do you find any experiment to the contrary?

A. Yes; for if you remove one of the plummets from the perpendicular, so as, for example, to make an angle with the perpendicular of eighty degrees, and the other so as to make an angle of sixty

blems
ravity.

degrees; they will not make their turns and returns in equal times.

B. And what say you is the cause of this?

A. Because the arches are the spaces which these two motions describe, they must be in double proportion to their own times: which cannot be, unless they be let go from equal altitudes, that is, from equal angles.

B. It is right; and the experiment does not cross, but confirm the equality of the times in all the arches they describe, even from ninety degrees to the least part of one degree.

A. But is it not too bold, if not extravagant an assertion, to say the earth is moved as a man shakes a basin or a sieve? Does not the earth move from west to east every day once, upon its own centre; and in the ecliptic circle once a-year? And now you give it another odd motion. How can all these consist in one and the same body?

B. Well enough. If you be a shipboard under sail, do not you go with the ship? Cannot you also walk upon the deck? Cannot every drop of blood move at the same time in your veins? How many motions now do you assign to one and the same drop of blood? Nor is it so extravagant a thing to attribute to the earth this kind of motion; but that I believe, if we certainly knew what motion it is that causeth the descent of bodies, we should find it either the same, or more extravagant. But seeing it can be nothing above that worketh this effect, it must be the earth itself that does it; and if the earth, then you can imagine no other motion to do it withal but this. And you will wonder more, when by the same motion I

Problems of gravity.

shall give you a probable account of the causes of very many other works of nature.

A. But what part of the heaven do you suppose the poles of your pricked circle point to?

B. I suppose them to be the same with the poles of the ecliptic. For, seeing the axis of the earth in this motion and in the annual motion, keeps parallel to itself, the axis must in both motions be parallel as to sense. For the circle which the earth describes, is not of visible magnitude at the distance it is from the sun.

A. Though I understand well enough how the earth may make a stone descend very swiftly under the ecliptic, or not far from it, where it throws off the air perpendicularly; yet about the poles of the circle methinks, it should cast off the air very weakly. I hope you will not say, that bodies descend faster in places remote from the poles, than nearer to them.

B. No; but I ascribe it to the like motion in the sun and moon. For such motions meeting, must needs cast the stream of the air towards the poles; and then there will be the same necessity for the descent there, that there is in other places, though perhaps a little more slowly. For you may have observed, that when it snows in the south parts, the flakes of snow are not so great as in the north: which is a probable sign they fall in the south from a greater height, and consequently disperse themselves more, as water does that falls down from a high and steep rock.

A. It is not improbable.

B. In natural causes all you are to expect, is but probability; which is better yet, than making

Problems of gravity.

gravity the cause, when the cause of gravity is that which you desire to know; and better than saying the earth draws it, when the question is, how it draws.

A. Why does the earth cast off air more easily than it does water, or any other heavy bodies?

B. It is indeed the earth that casteth off that air which is next unto it; but it is that air which casteth off the next air; and so continually, air moveth air; which it can more easily do than any other thing, because like bodies are more susceptible of one another's motions: as you may see in two lute-strings equally strained, what motion one string being stricken communicates to the air, the same will the other receive from the air; but strained to a differing note, will be less or not at all moved. For there is no body but air, that hath not some internal, though invisible, motion of its parts: and it is that internal motion which distinguisheth all natural bodies one from another.

A. What is the cause why certain squibs, though their substance be either wood or other heavy matter, made hollow and filled with gunpowder, which is also heavy; do nevertheless, when the gunpowder is kindled, fly upwards?

B. The same that keeps a man that swims from sinking, though he be heavier than so much water. He keeps himself up, and goes forward, by beating back the water with his feet; and so does a squib, by beating down the air with the stream of fired gunpowder, that proceeding from its tail makes it recoil.

A. Why does any brass or iron vessel, if it be hollow, float upon the water, being so very heavy?

Problems of gravity.

B. Because the vessel and the air in it, taken as one body, is more easily cast off than a body of water equal to it.

A. How comes it to pass, that a fish, (especially such a broad fish as a turbot or a plaice, which are broad and thin), in the bottom of the sea, perhaps a mile deep, is not pressed to death with the weight of water that lies upon the back of it?

B. Because all heavy bodies descend towards one point, which is the centre of the earth: and consequently the whole sea, descending at once, does arch itself so, as that the upper parts cannot press the parts next below them.

A. It is evident; nor can there possibly be any weight, as some suppose there is, of a cylinder of air or water or any other liquid thing, while it remains in its own element, or is sustained and inclosed in a vessel by which one part cannot press the other.

CHAPTER II.

PROBLEMS OF TIDES.

A. WHAT makes the flux and reflux of the sea, twice in a natural day?

B. We must come again to our basin of water; wherein you have seen, whilst it was moved, how the water mounteth up by the sides, and withal goes circling round about. Now if you should fasten to the inside of the basin some bar from the bottom to the top, you would see the water, instead of going on, go back again from that bar ebbing, and the water on the other side of the bar to do the same, but in counter-time; and consequently to be highest where the contrary streams meet together; and then return again, marking out four quarters of the vessel; two by their meeting, which are the high waters; and two by their retiring, which are the low waters.

A. What bar is that you find in the ocean that stops the current of the water, like that you make in the basin?

B. You know that the main ocean lies east and west, between India and the coast of America; and again on the other side, between America and India. If therefore the earth have such a motion as I have supposed, it must needs carry the current of the sea east and west: in which course, the bar that stoppeth it, is the south part of America, which leaves no passage for the water but the narrow strait of Magellan. The tide rises therefore upon the coast of America; and the rising of the same in this part of the world, proceedeth from the swelling chiefly of the water there, and partly also from the North Sea; which lieth also east and west, and has a passage out of the South Sea by the strait of Anian, between America and Asia.

A. Does not the Mediterranean Sea lie also east and west? Why are there not the like tides there?

B, So there are, proportionable to their lengths and quantity of water.

A. At Genoa, at Ancona, there are none at all, or not sensible.

B. At Venice there are, and in the bottom of the straits, and a current all along both the Mediterranean Sea and the Gulf of Venice: and it is

Problems of tides.

the current that makes the tides insensible at the sides; but the check makes them visible at the bottom.

A. How comes it about that the moon hath such a stroke in the business, as so sensibly to increase the tides at full and change?

B. The motion I have hitherto supposed but in the earth, I suppose also in the moon, and in all those great bodies that hang in the air constantly, I mean the stars, both fixed and errant. And for the sun and moon, I suppose the poles of their motion to be the poles of the equinoxial. Which supposed, it will follow (because the sun, the earth, and the moon, at every full and change are almost in one straight line) that this motion of the earth will then be made swifter than in the quarters. For this motion of the sun and moon being communicated to the earth, that hath already the like motion, maketh the same greater; and much greater when they are all three in one straight line, which is only at the full and change, whose tides are therefore called spring tides.

A. But what then is the cause that the spring tides themselves are twice a-year, namely, when the sun is in the equinoxial, greater than at any other times?

B. At other times of the year, the earth being out of the equinoxial, the motion thereof, by which the tides are made, will be less augmented, by so much as a motion in the obliquity of twenty-three degrees, or thereabout, which is the distance between the equinoxial and ecliptic circles, is weaker than the motion which is without obliquity.

A. All this is reasonable enough, if it be pos-

Problems of tides.

sible that such motions as you suppose in these bodies, be really there. But that is a thing I have some reason to doubt of. For the throwing off of air, consequent to these motions, is the cause, you say, that other things come to the earth; and therefore the like motions in the sun and moon and stars, casting off the air, should also cause all other things to come to every one of them. From whence it will follow, that the sun, moon, and earth, and all other bodies but air, should presently come together into one heap.

B. That does not follow. For if two bodies cast off the air, the motion of that air will be repressed both ways, and diverted into a course towards the poles on both sides; and then the two bodies cannot possibly come together.

A. It is true. And besides, this driving of the air on both sides, north and south, makes the like motion of air there also. And this may answer the question, how a stone could fall to the earth under the poles of the ecliptic, by the only casting off of air?

B. It follows from hence, that there is a certain and determinate distance of one of these bodies, the stars, from another, without any very sensible variation.

A. All this is probable enough, if it be true that there is no vacuum, no place empty in all the world. And supposing this motion of the sun and moon to be in the plain of the equinoxial, methinks :hat this should be the cause of the diurnal motion ' the earth; and because this motion of the earth you say, in the plain of the equinoxial, the ae should cause also a motion in the moon on

Problems of tides.

her own centre, answerable to the diurnal motion of the earth.

B. Why not? What else can you think makes the diurnal motion of the earth but the sun? And for the moon, if it did not turn upon its own centre, we should see sometimes one, sometimes another face of the moon, which we do not.

CHAPTER III.

PROBLEMS OF VACUUM.

A. WHAT convincing argument is there to prove, that in all the world there is no empty place?

B. Many; but I will name but one; and that is, the difficulty of separating two bodies hard and flat laid one upon another. I say the difficulty, not the impossibility. It is possible, without introducing vacuum, to pull asunder any two bodies, how hard and flat soever they be, if the force used be greater than the resistance of the hardness. And in case there be any greater difficulty to part them, besides what proceeds from their hardness, than there is to pull them further asunder when they are parted, that difficulty is argument enough to prove there is no vacuum.

A. These assertions need demonstration. And first, how does the difficulty of separation argue the plenitude of all the rest of the world?

B. If two flat polished marbles lie one upon another, you see they are hardly separated in all points at one and the same instant; and yet the weight of either of them is enough to make them slide off one from the other. Is not the

Problems of vacuum.

cause of this, that the air succeeds the marble that so slides, and fills up the place it leaves?

A. Yes, certainly. What then?

B. But when you pull the whole superficies asunder, not without great difficulty, what is the cause of that difficulty?

A. I think, as most men do, that the air cannot fill up the space between in an instant; for the parting is in an instant.

B. Suppose there be vacuum in that air into which the marble you pull off is to succeed, shall there be no vacuum in the air that was round about the two marbles when they touched? Why cannot that vacuum come into the place between? Air cannot succeed in an instant, because a body, and consequently cannot be moved through the least space in an instant. But emptiness is not a body, nor is moved, but is made by the act itself of separation. There is therefore, if you admit vacuum, no necessity at all for the air to fill the space left in an instant. And therefore, with what ease the marble coming off presseth out the vacuum of the air behind it, with the same ease will the marbles be pulled asunder. Seeing then, if there were vacuum, there would be no difficulty of separation, it follows, because there is difficulty of separation, that there is no vacuum.

A. Well, now, supposing the world full, how do you prove it possible to pull those marbles asunder?

B. Take a piece of soft wax; do not you think the one half touches the other half as close as the smoother [illegible]? Yet you can pull them [illegible] Still as you pull, the wax

Problems of vacuum.

grows continually more and more slender; there being a perpetual parting or discession of the outermost part of the wax one from another, which the air presently fills; and so there is a continual lessening of the wax, till it be no bigger than a hair, and at last separation. If you can do the same to a pillar of marble, till the outside give way, the effect will be the same, but much quicker, after it once begins to break in the superficies; because the force that can master the first resistance of the hardness, will quickly dispatch the rest.

A. It seems so by the brittleness of some hard bodies. But I shall afterward put some questions to you, touching the nature of hardness. But now to return to our subject. What reason can you render (without supposing vacuum) of the effects produced in the engine they use at Gresham college?

B. That engine produceth the same effects that a strong wind would produce in a narrow room.

A. How comes the wind in? You know the engine is a hollow round pipe of brass, into which is thrust a cylinder of wood covered with leather, and fitted to the cylinder so exactly as no air can possibly pass between the leather and the brass?

B. I know it; and that they may thrust it up, there is a hole left in the cylinder to let the air out before it, which they can stop when they please. There is also in the bottom of the cylinder a passage into a hollow globe of glass, which passage they can also open and shut at pleasure. And at the top of that globe there is a wide mouth to put in what they please to try conclusions on, and that also to be opened and shut as shall be needful. It is of the nature of a pop-gun which children use,

Problems of vacuum.

but great, costly, and more ingenious. They thrust forward and pull back the wooden cylinder (because it requires much strength) with an iron screw. What is there in all this to prove the possibility of vacuum.

A. When this wooden cylinder covered with leather, fit and close, is thrust home to the bottom, and the holes in the hollow cylinder of brass close stopped, how can it be drawn back, as with the screw they draw it, but that the space it leaves must needs be empty: for it is impossible that any air can pass into the place to fill it?

B. Truly I think it close enough to keep out straw and feathers, but not to keep out air, nor yet matter. For suppose they were not so exactly close but that there were round about a difference for a small hair to lie between; then will the pulling back of the cylinder of wood force so much air in, as in retiring it forces back, and that without any sensible difficulty. And the air will so much more swiftly enter as the passage is left more narrow. Or if they touch, and the contact be in some points and not in all, the air will enter as before, in case the force be augmented accordingly. Lastly, though they touch exactly, if either the leather yield, or the brass, which it may do, to the force of a strong screw, the air will again enter. Do you think it possible to make two superficies so exquisitely touch in all points as you suppose, or leather so hard as not to yield to the force of a screw? The body of leather will give passage both to air and water, as you will confess when you ride in rainy and windy weather. You may therefore be assured that in drawing out their

Problems of vacuum.

wooden leather cylinder, they force in as much air as will fill the place it leaves, and that with as much swiftness as is answerable to the strength that drives it in. The effect therefore of their pumping is nothing else but a vehement wind, a very vehement wind, coming in on all sides of the cylinder at once into the hollow of the brass pipe, and into the hollow of the glass globe joined to it.

A. I see the reason already of one of their wonders, which is, that the cylinder they pump with, if it be left to itself, after it is pulled back, will swiftly go up again. You will say the air comes out again with the same violence by reflection, and I believe it.

B. This is argument enough that the place was not empty. For what can fetch or drive up the sucker, as they call it, if the place within were empty? For that there is any weight in the air to do it, I have already demonstrated to be impossible. Besides, you know, when they have sucked out, as they think, all the air from the glass globe, they can nevertheless both see through it what is done, and hear a sound from within when there is any made; which, if there were no other, but there are many other, is argument enough that the place is still full of air.

A. What say you to the swelling of a bladder even to bursting, if it be a little blown when it is put into the receiver, for so they call the globe of glass?

B. The streams of air that from every side meeting together, and turning in an infinite number of small points, do pierce the bladder in innumerable places with great violence at once, like so many invisible small wimbles, especially if the bladder be a little blown before it be put in, that it may make a little resistance. And when the air has once pierced it, it is easy to conceive, that it must afterward by the same violent motion be extended till it break. If before it break you let in fresh air upon it, the violence of the motion will thereby be tempered, and the bladder be less extended; for that also they have observed. Can you imagine how a bladder should be extended and broken by being too full of emptiness?

A. How come living creatures to be killed in this receiver, in so little a time as three or four minutes of an hour?

B. If they suck into their lungs so violent a wind thus made, you must needs think it will presently stop the passage of their blood; and that is death; though they may recover if taken out before they be too cold. And so likewise will it put out fire; but the coals taken out whilst they are hot will revive again. It is an ordinary thing in many coal-pits, whereof I have seen the experience, that a wind proceeding from the sides of the pit every way, will extinguish any fire let down into it, and kill the workmen, unless they be quickly taken out.

A. If you put a vessel of water into the receiver, and then suck out the air, the water will boil; what say you to that?

B. It is like enough it will dance in so great a bustling of the air; but I never heard it would be hot. Nor can I imagine how vacuum should make anything dance. I hope you are by this time satisfied, that no experiment made with the engine at

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Gresham College, is sufficient to prove that there is, or that there may be vacuum.

A. The world you know is finite, and consequently, all that infinite space without it is empty. Why may not some of that vacuum be brought in, and mingled with the air here?

B. I know nothing in matters without the world.

A. What say you to Torricellio's experiment in quicksilver, which is this: there is a basin at A filled with quicksilver, suppose to B, and C D a hollow glass pipe filled with the same, which if you stop with your finger at B, and so set it upright, and then if you take away your finger, the quicksilver will fall from C downwards but not to the bottom, for it will stop by the way, suppose at D. Is it not therefore necessary that that space between C and D be left empty? Or will you say the quicksilver does not exactly touch the sides of the glass pipe?

B. I will say neither. If a man thrust down into a vessel of quicksilver a blown bladder, will not that bladder come up to the top?

A. Yes, certainly, or a bladder of iron, or anything else but gold.

B. You see then that air can pierce quicksilver.

A. Yes, with so much force as the weight of quicksilver comes to.

B. When the quicksilver is fallen to D, there is so much the more in the basin, and that takes up the place which so much air took up before. Whither can this air go if all the world without that glass pipe B C were full? There must needs be the same or as much air come into that space,

Problems of vacuum.

which only is empty, between C and D: by what force? By the weight of the quicksilver between D and B. Which quicksilver weigheth now upward, or else it could never have raised that part higher, which was at first in the basin. So you see the weight of quicksilver can press the air through quicksilver up into the pipe, till it come to an equality of force as in D, where the weight of the quicksilver is equal to the force which is required in air to go through it.

A. If a man suck a phial that has nothing in it but air, and presently dip the mouth of it into water, the water will ascend into the phial. Is not that an argument that part of the air had been sucked out, and part of the room within the phial left empty?

B. No. If there were empty space in the world, why should not there be also some empty space in the phial before it was sucked? And then why does not the water rise to fill that? When a man sucks the phial he draws nothing out, neither into his belly, nor into his lungs, nor into his mouth; only he sets the air within the glass into a circular motion, giving it at once an endeavour to go forth by the sucking, and an endeavour to go back by not receiving it into his mouth; and so with a great deal of labour glues his lips to the neck of the phial. Then taking it off, and dipping the neck of the phial into the water before the circulation ceases, the air, with the endeavour it hath now gotten, pierces the water and goes out: and so much air as goes out, so much matter comes up into the room of it.

CHAPTER IV.

PROBLEMS OF HEAT AND LIGHT.

CHAP. IV.
Problems of heat and light.

A. WHAT is the cause of heat?

B. How know you, that any thing is hot but yourself?

A. Because I perceive by sense it heats me.

B. It is no good argument, the thing heats me; therefore it is hot. But what alteration do you find in your body at any time by being hot?

A. I find my skin more extended in summer than in winter; and am sometimes fainter and weaker then ordinary, as if my spirits were exhaled; and I sweat.

B. Then that is it you would know the cause of. I have told you before that by the motion I suppose both in the sun, and in the earth, the air is dissipated, and consequently that there would be an infinite number of small empty places, but that the world being full, there comes from the next parts other air into the spaces they would else make empty. When therefore this motion of the sun is exercised upon the superficies of the earth, if there do not come out of the earth itself some corporal substance to supply that tearing of the air, we must return again to the admission of vacuum. If there do, then you see how by this motion fluid bodies are made to exhale out of the earth. The like happens to a man's body or hand, which when he perceives, he says he is hot. And so of the earth when it sendeth forth water and earth together in plants, we say it does it by heat from the sun.

A. It is very probable, and no less probable, that the same action of the sun is that which from the sea and moist places of the earth, but especially from the sea, fetcheth up the water into the clouds. But there be many ways of heating besides the action of the sun or of fire. Two pieces of wood will take fire if in turning they be pressed together.

B. Here again you have a manifest laceration of the air by the reciprocal and contrary motions of the two pieces of wood, which necessarily causeth a coming forth of whatsoever is aereal or fluid within them, and (the motion pursued) a dissipation also of the other more solid parts into ashes.

A. How comes it to pass that a man is warmed even to sweating, almost with every extraordinary labour of his body?

B. It is easy to understand, how by that labour all that is liquid in his body is tossed up and down, and thereby part of it also cast forth.

A. There be some things that make a man hot without sweat or other evaporation, as caustics, nettles, and other things.

B. No doubt. But they touch the part they so heat, and cannot work that effect at any distance.

A. How does heat cause light, and that partially, in some bodies more, in some less, though the heat be equal?

B. Heat does not cause light at all. But in many bodies, the same cause, that is to say, the same motion, causeth both together; so that they are not to one another as cause and effect, but are

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concomitant effects sometimes of one and the same motion.

A. How?

B. You know the rubbing or hard pressing of the eye, or a stroke upon it, makes an apparition of light without and before it, which way soever you look. This can proceed from nothing else but from the restitution of the organ pressed or stricken, unto its former ordinary situation of parts. Does not the sun by his thrusting back the air upon your eyes press them? Or do not those bodies whereon the sun shines, though by reflection, do the same, though not so strongly? And do not the organs of sight, the eye, the heart, and brains, resist that pressure by an endeavour of restitution outwards? Why then should there not be without and before the eye, an apparition of light in this case as well as in the other?

A. I grant there must. But what is that which appears after the pressing of the eye? For there is nothing without that was not there before; or if there were, methinks another should see it better, or as well as he; or if in the dark, methinks it should enlighten the place.

B. It is a fancy, such as is the appearance of your face in a looking-glass; such as is a dream; such as is a ghost; such as is a spot before the eye that hath stared upon the sun or fire. For all these are of the regiment of fancy, without any body concealed under them, or behind them, by which they are produced.

A. And when you look towards the sun or moon, why is not that also which appears before your eyes at that time a fancy?

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B. So it is. Though the sun itself be a real body, yet that bright circle of about a foot diameter cannot be the sun, unless there be two suns, a greater and a lesser. And because you may see that which you call the sun, both above you in the sky, and before you in the water, and two suns, by distorting your eye, in two places in the sky, one of them must needs be fancy. And if one, both. All sense is fancy, though the cause be always in a real body.

A. I see by this that those things which the learned call the accidents of bodies, are indeed nothing else but diversity of fancy, and are inherent in the sentient, and not in the objects, except motion and quantity. And I perceive by your doctrine you have been tampering with *Leviathan.* But how comes wood with a certain degree of heat to shine, and iron also with a greater degree; but no heat at all to be able to make water shine?

B. That which shineth hath the same motion in its parts that I have all this while supposed in the sun and earth. In which motion there must needs be a competent degree of swiftness to move the sense, that is, to make it visible. All bodies that are not fluid will shine with heat, if the heat be very great. Iron will shine and gold will shine; but water will not, because the parts are carried
ore they attain to that degree of swiftness
quisite.

are many fluid bodies whose parts
and yet they make a flame, as oil, and
ther strong drinks.

oil I never saw any inflamed by itself,
ever heated, therefore I do not think

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they are the parts of the oil, but of the combustible body oiled that shine; but the parts of wine and strong drinks have partly a strong motion of themselves, and may be made to shine, but not with boiling, but by adding to them as they rise the flame of some other body.

A. How can it be known that the particles of wine have such a motion as you suppose?

B. Have you ever been so much distempered with drinking wine, as to think the windows and table move?

A. I confess, though you be not my confessor, I have; but very seldom; and I remember the window seemed to go and come in a kind of circling motion, such as you have described. But what of that?

B. Nothing, but that it was the wine that caused it; which having a good degree of that motion before, did, when it was heated in the veins, give that concussion, which you thought was in the window, to the veins themselves, and, by the continuation of the parts of man's body, to the brain; and that was it which made the window seem to move.

A. What is flame? For I have often thought the flame that comes out of a small heap of straw to be more, before it hath done flaming, than a hundred times the straw itself.

B. It was but your fancy. If you take a stick in your hand by one end, the other end burning, and move it swiftly, the burning end, if the motion be circular, shall seem a circle; if straight, a straight line of fire, longer or shorter, according to the swiftness of the motion, or the space it moves in. You know the cause of that.

A. I think it is, because the impression of that visible object, which was made at the first instant of the motion, did last till it was ended. For then it will follow that it must be visible all the way, the impressions in all points of the time being equal.

B. The cause can be no other. The smallest spark of fire flying up seems a line drawn upward; and again by that swift circular motion which we have supposed for the cause of light, seems also broader than it is. And consequently the flame of every thing must needs seem much greater than it is.

A. What are those sparks that fly out of the fire?

B. They are small pieces of the wood or coals, or other fuel loosened and carried away with the air that cometh up with them. And being extinguished before their parts be quite dissipated into others, are so much soot, and black, and may be fired again.

A. A spark of fire may be stricken out of a cold stone. It is not therefore heat that makes this shining.

B. No it is the motion that makes both the heat and shining; and the stroke makes the motion. For every of those sparks, is a little parcel of the stone, which swiftly moved, imprinteth the same motion into the matter prepared, or fit to receive it.

A. How comes the light of the sun to burn almost any combustible matter by refraction through a convex glass, and by reflection from a concave?

B. The air moved by the sun presseth the con-

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vex glass in such manner as the action continued through it, proceedeth not in the same straight line by which it proceeded from the sun, but tendeth more toward the centre of the body it enters. Also when the action is continued through the convex body, it bendeth again the same way. By which means the whole action of the sun-beams are enclosed within a very small compass: in which place therefore there must be a very vehement motion; and consequently, if there be in that place combustible matter, such as is not very hard to kindle, the parts of it will be dissipated, and receive that motion which worketh on the eye as other fire does.

The same reason is to be given for burning by reflection. For there also the beams are collected into almost a point.

A. Why may not the sun-beams be such a body as we call fire, and pass through the pores of the glass so disposed as to carry them to a point, or very near?

B. Can there be a glass that is all pores? if there cannot, then cannot this effect be produced by the passing of fire through the pores. You have seen men light their tobacco at the sun with a burning glass, or with a ball of crystal, held which way they will indifferently. Which must be impossible, unless the ball were all pores. Again, neither you nor I can conceive any other fire than we have seen, nor than such as water will put out. But not only a solid globe of glass or crystal will serve for a burning-glass, but also a hollow one filled with water. How then does the fire from the sun pass through the glass of water without being

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put out before it come to the matter they would have it burn?

A. I know not. There comes nothing from the sun. If there did, there is come so much from it already, that at this day we had had no sun.

CHAPTER V.

PROBLEMS OF HARD AND SOFT.

A. WHAT call you hard, and what soft?

B. That body whereof no one part is easily put out of its place, without removing the whole, is that which I and all men call hard; and the contrary soft. So that they are but degrees one of another.

A. What is the cause that makes one body harder than another, or, seeing you say they are but degrees of one another, what makes one body softer than another, and the same body sometimes harder, sometimes softer?

B. The same motion which we have supposed from the beginning for the cause of so many other effects. Which motion not being upon the centre of the part moved, but the part itself going in another circle to and again, it is not necessary that the motion be perfectly circular. For it is not circulation, but the reciprocation, I mean the to and again, that does cast off, and lacerate the air, and consequently produce the fore-mentioned effects.

For the cause therefore of hardness, I suppose the reciprocation of motion in those things which are hard, to be very swift, and in very small circles.

Problems of hard and soft.

A. This is somewhat hard to believe. I would you could supply it with some visible experience.

B. When you see, for example, a cross-bow bent, do you think the parts of it stir?

A. No. I am sure they do not.

B. How are you sure? You have no argument for it, but that you do not see the motion. When I see you sitting still, must I believe there is no motion in your parts within, when there are so many arguments to convince me there is.

A. What argument have you to convince me that there is motion in a cross-bow when it stands bent?

B. If you cut the string, or any way set the bow at liberty, it will have then a very visible motion. What can be the cause of that?

A. Why the setting of the bow at liberty.

B. If the bow had been crooked before it was bent, and the string tied to both ends, and then cut asunder, the bow would not have stirred. Where lies the difference?

A. The bow bent has a spring; unbent it has none, how crooked soever.

B. What mean you by spring?

A. An endeavour of restitution to its former posture.

B. I understand spring as well as I do endeavour.

A. I mean a principle or beginning of motion in a contrary way to that of the force which bent it.

B. But the beginning of motion is also motion, how insensible soever it be. And you know that nothing can give a beginning of motion to itself. What is it therefore that gives the bow (which you say you are sure was at rest when it stood bent)

Problems of hard and soft.

its first endeavour to return to its former posture?

A. It was he that bent it.

B. That cannot be. For he gave it an endeavour to come forward, and the bow endeavours to go backward.

A. Well, grant that endeavour be motion, and motion in the bow unbent, how do you derive from thence, that being set at liberty it must return to its former posture?

B. Thus there being within the bow a swift (though invisible) motion of all the parts, and consequently of the whole; the bending causeth that motion, which was along the bow (that was beaten out when it was hot into that length) to operate across the length in every part of it, and the more by how much it is more bent; and consequently endeavours to unbend it all the while it stands bent. And therefore when the force which kept it bent is removed, it must of necessity return to the posture it had before.

A. But has that endeavour no effect at all before the impediment be removed? For if endeavour be motion, and every motion have some effect more or less, methinks this endeavour should in time produce something.

B. So it does. For in time (in a long time) the course of this internal motion will lie along the bow, not according to the former, but to the new acquired posture. And then it well be as uneasy to return it to its former posture, as it was before to bend it.

A. That is true. For bows long bent lose their appetite to restitution, long custom becoming

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nature. But from this internal reciprocation of the parts, how do you infer the hardness of the whole body?

B. If you apply force to any single part of such a body, you must needs disorder the motion of the next parts to it before it yield, and there disordered, the motion of the next again must also be disordered; and consequently no one part can yield without force sufficient to disorder all: but then the whole body must also yield. Now when a body is of such a nature as no single part can be removed without removing the whole, men say that body is hard.

A. Why does the fire melt divers hard bodies, and yet not all?

B. The hardest bodies are those wherein the motion of the parts are the most swift, and yet in the least circles. Wherefore if the fire, the motion of whose parts are swift, and in greater circles, be made so swift, as to be strong enough to master the motion of the parts of the hard body, it will make those parts to move in a greater compass, and thereby weaken their resistance, that is to say, soften them, which is a degree of liquefaction. And when the motion is so weakened, as that the parts lose their coherence by the force of their own weight, then we count the body melted.

A. Why are the hardest things the most brittle, insomuch that what force soever is enough to bend them, is enough also to break them?

B. In bending a hard body, as (for example) a rod of iron, you do not enlarge the space of the internal motion of the parts of iron, as the fire

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does; but you master and interrupt the motion, and that chiefly in one place. In which place the motion that makes the iron hard being once overcome, the prosecution of that bending must needs suddenly master the motions of the parts next unto it, being almost mastered before.

A. I have seen a small piece of glass, the figure whereof is this, A A B C. Which piece of glass if you bend toward the top, as in C, the whole body will shatter asunder into a million of pieces, and be like to so much dust. I would fain see you give a probable reason of that.

B. I have seen the experiment. The making of the glass is thus: they dip an iron rod into the molten glass that stands in a vessel within the furnace. Upon which iron rod taken out, there will hang a drop of molten but tough metal of the figure you have described, which they let fall into the water. So that the main drop comes first to the water, and after it the tail, which though straight whilst it hung on the end of the rod, yet by falling into the water becomes crooked. Now you know the making of it, you may consider what must be the consequence of it. Because the main drop A comes first to the water, it is therefore first quenched, and consequently the motion of the parts of that drop, which by the fire were made to
oved in a larger compass, is by the water
shrink into lesser circles towards the other
but with the same or not much less swiftness.

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you take any long piece of iron, glass, or
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your hand, and so quench it suddenly, though before you held it easily enough, yet now it will burn your fingers.

A. It will so.

B. You see then how the motion of the parts from A toward C is made more violent and in less compass by quenching the other parts first. Besides, the whole motion that was in all the parts of the main drop A, is now united in the small end B C. And this I take to be the cause why that small part B C is so exceeding stiff. Seeing also this motion in every small part of the glass, is not only circular, but proceeds also all along the glass from A to B, the whole motion compounded will be such as the motion of spinning any soft matter into thread, and will dispose the whole body of the glass in threads, which in other hard bodies are called the grain. Therefore if you bend this body (for example) in C (which to do will require more force than a man would think that has not tried) those threads of glass must needs be all bent at the same time, and stand so, till by the breaking of the glass at C, they be all at once set at liberty; and then all at once being suddenly unbent, like so many brittle and overbent bows, their strings breaking, be shivered in pieces.

A. It is like enough to be so. And if nature have betrayed herself in any thing, I think it is in this, and in that other experience of the cross-bow; which strongly and evidently demonstrates the internal reciprocation of the motion, which you suppose to be in the internal parts of every hard body. And I have observed somewhat in looking-glasses which much confirms that there is some

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such motion in the internal parts of glass, as you have supposed for the cause of hardness. For let the glass be A B, and let the object at C be a candle, and the eye at D. Now by divers reflections and refractions in the two superficies of the glass, if the lines of vision be very oblique, you shall see many images of the candle, as E, F, G, in such order and position as is here described. But if you remove your eye to C, and the candle to D, they will appear in a situation manifestly different from this. Which you will yet more plainly perceive if the looking-glass be coloured, as I have observed in red and blue glasses; and could never conceive any probable cause of it, till now you tell me of this secret motion of the parts across the grain of the glass, acquired by cooling it this or that way.

B. There be very many kinds of hard bodies, metals, stones, and other kinds, in the bowels of the earth, that have been there ever since the beginning of the world; and I believe also many different sorts of juices that may be made hard. But for one general cause of hardness it can be no other than such an internal motion of parts as I have already described, whatsoever may be the cause of the several concomitant qualities of their hardness in particular.

A. We see water hardened every frosty day. It is likely therefore you may give a probable cause of ice. What is the cause of freezing of the ocean towards the poles of the earth?

B. You know the sun being always between the tropics, and (as we have supposed) always casting off the air; and the earth likewise casting it off

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from itself, there must needs on both sides be a great stream of air towards the poles, shaving the superficies of the earth and sea, in the northern and southern climates. This shaving of the earth and sea by the stream of air must needs contract and make to shrink those little circles of the internal parts of earth and water, and consequently harden them, first at the superficies, into a thin skin, which is the first ice; and afterwards the same motion continuing, and the first ice co-operating, the ice becomes thicker. And this I conceive to be the cause of the freezing of the ocean.

A. If that be the cause, I need not ask how a bottle of water is made to freeze in warm weather with snow, or ice mingled with salt. For when the bottle is in the midst of it, the wind that goeth out both of the salt and of the ice as they dissolve, must needs shave the superficies of the bottle, and the bottle work accordingly on the water without it, and so give it first a thin skin, and at last thicken it into a solid piece of ice. But how comes it to pass that water does not use to freeze in a deep pit?

B. A deep pit is a very thick bottle, and such as the air cannot come at but only at the top, or where the earth is very loose and spungy.

A. Why will not wine freeze as well as water?

B. So it will when the frost is great enough. But the internal motion of the parts of wine and other heating liquors is in greater circles and stronger than the motion of the parts of water; and therefore less easily to be frozen, especially quite through, because those parts that have the strongest motion retire to the centre of the vessel.

CHAPTER VI.

PROBLEMS OF RAIN, WIND, AND OTHER WEATHER.

CHAP. VI.

Problems of rain, wind, and other weather.

A. WHAT is the original cause of rain? And how is it generated?

B. The motion of the air (such as I have described to you already) tending to the disunion of the parts of the air, must needs cause a continual endeavour (there being no possibility of vacuum) of whatsoever fluid parts there are upon the face of the earth and sea, to supply the place which would else be empty. This makes the water, and also very small and loose parts of the earth and sea to rise, and mingle themselves with the air, and to become mist and clouds. Of which the greatest quantity arise there, where there is most water, namely, from the large parts of the ocean; which are the South Sea, the Indian Sea, and the sea that divided Europe and Africa from America; over which the sun for the greatest part of the year is perpendicular, and consequently raiseth a greater quantity of water; which afterwards gathered into clouds, falls down in rain.

A. If the sun can thus draw up the water, though but in small drops, why can it not as easily hold it up?

B. It is likely it would also hold them up, if they did not grow greater by meeting together, nor were carried away by the air towards the poles.

A. What makes them gather together?

B. It is not improbable that they are carried

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against hills, and there stopt till more overtake them. And when they are carried towards the North or South where the force of the sun is more oblique and thereby weaker, they descend gently by their own weight. And because they tend all to the centre of the earth, they must needs be united in their way for want of room, and so grow bigger. And then it rains.

A. What is the reason it rains so seldom, but snows so often upon very high mountains?

B. Because, perhaps, when the water is drawn up higher than the highest mountains, where the course of the air between the equator and the poles is free from stoppage, the stream of the air freezeth it into snow. And it is in those places only where the hills shelter it from that stream, that it falls in rain.

A. Why is there so little rain in Egypt, and yet so much in other parts nearer the equinoxial, as to make the Nile overflow the country?

B. The cause of the falling of rain I told you was the stopping, and consequently the collection of clouds about great mountains, especially when the sun is near the equinoxial, and thereby draws up the water more potently, and from greater seas. If you consider therefore that the mountains in which are the springs of Nile, lie near the equinoxial and are exceedingly great, and near the Indian Sea, you will not think it strange there should be great store of snow. This as it melts makes the rain of Nile to rise, which in April and May going on toward Egypt arrive there about the time of the solstice, and overflow the country.

A. Why should not the Nile then overflow that

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country twice a year, for it comes twice a-year to the equinoxial.

B. From the autumnal equinox, the sun goeth on toward the southern tropic, and therefore cannot dissolve the snow on that side of the hills that looks towards Egypt.

A. But then there ought to be such another inundation southward.

B. No doubt but there is a greater descent of water there in their summer than at other times, as there must be wheresoever there is much snow melted. But what should that inundate, unless it should overflow the sea that comes close to the foot of those mountains? And for the cause why it seldom rains in Egypt, it may be this, that there are no very high hills near it to collect the clouds. The mountains whence Nile riseth being near two thousand miles off. The nearest on one side are the mountains of Nubia, and on the other side Sina and the mountains of Arabia.

A. Whence think you proceed the winds?

B. From the motion, I think, especially of the clouds, partly also from whatsoever is moved in the air.

A. It is manifest that the clouds are moved by the winds; so that there were winds before any clouds could be moved. Therefore I think you make the effect before the cause.

B. If nothing could move a cloud but wind, your objection were good. But you allow a cloud to descend by its own weight. But when it so descends, it must needs move the air before it, even to the earth, and the earth again repel it, and so make lateral winds every way, which will

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carry forward other clouds if there be any in their way, but not the cloud that made them. The vapour of the water rising into clouds, must needs also, as they rise, raise a wind.

A. I grant it. But how can the slow motion of a cloud make so swift a wind as it does?

B. It is not one or two little clouds, but many and great ones that do it. Besides, when the air is driven into places already covered, it cannot but be much the swifter for the narrowness of the passage.

A. Why does the south wind more often than any other bring rain with it?

B. Where the sun hath most power, and where the seas are greatest, that is in the south, there is most water in the air; which a south wind can only bring to us. But I have seen great showers of rain sometimes also when the wind hath been north, but it was in summer, and came first, I think, from the south or west, and was brought back from the north.

A. I have seen at sea very great waves when there was no wind at all. What was it then that troubled the water?

B. But had you not wind enough presently after?

A. We had a storm within a little more than a quarter of an hour after.

B. That storm was then coming and had moved the water before it. But the wind you could not perceive, for it came downwards with the descending of the clouds, and pressing the water bounded above your sail till it came very near. And that was it that made you think there was no wind at all.

A. How comes it to pass that a ship should go against the wind which moves it, even almost point blank, as if it were not driven but drawn?

B. You are to know first, that what body soever is carried against another body, whether perpendicularly or obliquely, it drives it in a perpendicular to the superficies it lighteth on. As for example, a bullet shot against a flat wall, maketh the stone, or other matter it hits, to retire in a perpendicular to that flat; or, if the wall be round, towards the centre, that is to say, perpendicularly. For if the way of the motion be oblique to the wall, the motion is compounded of two motions, one parallel to the wall, and the other perpendicular. By the former whereof the bullet is carried along the wall side, by the other it approacheth to it. Now the former of these motions can have no effect upon it; all the battery is from the motion perpendicular, in which it approacheth, and therefore the part it hits must also retire perpendicularly. If it were not so, a bullet with the same swiftness would execute as much obliquely shot, as perpendicularly, which you know it does not.

A. How do you apply this to a ship?

B. Let A B be the ship, the head of it A. If the wind blow just from A towards B, it is true the ship cannot go forward howsoever the sail be set. Let C D be perpendicular to the ship, and let the sail E C be never so little oblique to it, and F C perpendicular to E C, and then you see the ship will gain the space D F to the headward.

A. It will so; but when it is very near to the wind it will go forward very slowly, and make more way with her side to the leeward.

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B. It will indeed go slower in the proportion of the line A E to the line C E. But the ship will not go so fast as you think sideward: one cause is the force of that wind which lights on the side of the ship itself; the other is the bellying of the sail; for the former, it is not much, because the ship does not easily put from her the water with her side; and bellying of the sail gives some little hold for the wind to drive the ship astern.

A. For the motion sideward I agree with you; but I had thought the bellying of the sail had made the ship go faster.

B. But it does not; only in a fore wind it hinders least.

A. By this reason a broad thin board should make the best sail.

B. You may easily foresee the great incommodities of such a sail. But I have seen tried in little what such a wind can do in such a case. For I have seen a board set upon four truckles, with a staff set up in the midst of it for a mast, and another very thin and broad board fastened to that staff in the stead of a sail, and so set as to receive the wind very obliquely, I mean so as to be within a point of the compass directly opposite to it, and so placed upon a reasonable smooth pavement where the wind blew somewhat strongly. The event was first, that it stood doubting whether it should stir at all or no, but that was not long, and then it ran a-head extreme swiftly, till it was overthrown by a rub.

A. Before you leave the ship, tell me how it comes about that so small a thing as a rudder can so easily turn the greatest ship.

Problems of rain, wind, and other weather.

B. It is not the rudder only, there must also be a stream to do it; you shall never turn a ship with a rudder in a standing pool, nor in a natural current. You must make a stream from head to stern, either with oars or with sails; when you have made such a stream, the turning of the rudder obliquely holds the water from passing freely, and the ship or boat cannot go on directly, but as the rudder inclines to the stern, so will the ship turn; but this is too well known to insist upon. You have observed that the rudders of the greatest ships are not very broad, but go deep into the water, whereas western barges, though but small vessels, have their rudders much broader, which argues that the holding of water from passing is the true office of a rudder; and therefore to a ship that draws much water the rudder is made deep accordingly; and in barges that draw little water, the rudders being less deep, must so much the more be extended in breadth.

A. What makes snow?

B. The same cause which, speaking of hardness, I supposed for the cause of ice. For the stream of air proceeding from that both the earth and the sun cast off the air, consequently maketh a stream of air from the equinoxial towards the poles, passing amongst the clouds, shaving those small drops of water whereof the clouds consist, and congeals them as they do the water of the sea, or of a river. And these small frozen drops are that which we call snow.

A. But then how are great drops frozen into hailstones, and that especially (as we see they are) in summer?

Problems of rain, wind, and other weather.

B. It is especially in summer, and hot weather, that the drops of water which make the clouds, are great enough; but it is then also that clouds are sooner and more plentifully carried up. And therefore the current of the air strengthened between the earth and the clouds, becomes more swift; and thereby freezeth the drops of water, not in the cloud itself, but as they are falling. Nor does it freeze them thoroughly, the time of their falling not permitting it, but gives them only a thin coat of ice; as is manifest by their sudden dissolving.

A. Why are not sometimes also whole clouds when pregnant and ready to drop, frozen into one piece of ice?

B. I believe they are so whensoever it thunders.

A. But upon what ground do you believe it?

B. From the manner or kind of noise they make, namely a crack; which I see not how it can possibly be made by water or any other soft bodies whatsoever.

A. Yes, the powder they call *aurum fulminans*, when thoroughly warm, gives just such another crack as thunder.

B. But why may not every small grain of that *aurum fulminans* by itself be heard, though a heap of them together be soft, as is any heap of sand. Salts of all sorts are of the nature of ice. But gold is dissolved into *aurum fulminans* by nitre and other salts. And the least grain of it gives a little crack in the fire by itself. And therefore when they are so warmed by degrees, the crack cannot choose but be very great.

Problems of rain, wind, and other weather.

A. But before it be *aurum fulminans* they use to wash away the salt (which they call dulcifying it), and then they dry it gently by degrees.

B. That is, they exhale the pure water that is left in the powder, and leave the salt behind to harden with drying. Other powder made of salts without any gold in them will give a crack as great as *aurum fulminans*. A very great chemist of our times hath written, that salt of tartar, saltpetre, and a little brimstone ground together into a powder, and dried, a few grains of that powder will be made by the fire to give as great a clap as a musket.

A. Methinks it were worth your trial to see what effect a quart or a pint of *aurum fulminans* would produce, being put into a great gun made strong enough on purpose, and the breech of the gun set in hot cinders, so as to heat by degrees, till the powder fly.

B. I pray you try it yourself; I cannot spare so much money.

A. What is it that breaketh the clouds when they are frozen?

B. In very hot weather the sun raiseth from the sea and all moist places abundance of water, and to a great height. And whilst this water hangs over us in clouds, or is again descending, it raiseth other clouds, and it happens very often that they press the air between them, and squeeze it through the clouds themselves very violently; which as it passes shaves and hardens them in the manner declared.

A. That has already been granted; my question is what breaks them?

Problems of rain, wind, and other weather.

B. I must here take in one supposition more.

A. Then your basin, it seems, holds not all you have need of.

B. It may for all this, for the supposition I add is no more but this; that what internal motion I ascribe to the earth, and the other concrete parts of the world, is to be supposed also in every of their parts how small soever; for what reason is there to think, in case the whole earth have in truth the motion I have ascribed to it, that one part of it taken away, the remaining part should lose that motion. If you break a loadstone, both parts will retain their virtue, though weakened according to the diminution of their quantity; I suppose therefore in every small part of the earth the same kind of motion, which I have supposed in the whole: and so I recede not yet from my basin.

A. Let it be supposed, and withal, that abundance of earth, (which I see you aim at), be drawn up together with the water. What then?

B. Then if many pregnant clouds, some ascending and some descending meet together, and make concavities between, and by the pressing out of the air, as I have said before, become ice; those atoms, as I may call them, of earth will, by the straining of the air through the water of the clouds, be left behind, and remain in the cavities of the clouds, and be more in number than for the proportion of the air therein. Therefore for want of liberty they must needs justle one another, and become, as they are more and more straightened of room, more and more swift, and consequently at last break the ice suddenly and violently, now in one place, and by

Problems of rain, wind, and other weather.

and by in another; and make thereby so many claps of thunder, and so many flashes of lightning. For the air recoiling upon our eyes, is that which maketh those flashes to our fancy.

A. But I have seen lightning in a very clear evening, when there has been neither thunder nor clouds.

B. Yes, in a clear evening; because the clouds and the rain were below the horizon, perhaps forty or fifty miles off; so that you could not see the clouds nor hear the thunder.

A. If the clouds be indeed frozen into ice, I shall not wonder if they be sometimes also so situated, as, like looking-glasses, to make us see sometimes three or more suns by refraction and reflection.

CHAPTER VII.

PROBLEMS OF MOTION PERPENDICULAR, AND OBLIQUE; OF PRESSION AND PERCUSSION; REFLECTION AND REFRACTION; ATTRACTION AND REPULSION.

A. If a bullet from a certain point given, be shot against a wall perpendicularly, and again from the same point obliquely, what will be the proportion of the forces wherewith they urge the wall? For example, let the wall be A B, a point given E, a gun C E, that carries the bullet perpendicularly to F, and another gun D E, that carries the like bullet with the same swiftness obliquely to G; in what proportion will their forces be upon the wall?

B. The force of the stroke perpendicular from E to F will be greater then the oblique force from E to G, in the proportion of the line E G to the line E F.

Problems of motion perpendicular, and oblique; of pression and percussion; reflection and refraction; attraction and repulsion.

A. How can the difference be so much? Can the bullet lose so much of its force in the way from E to G?

B. No; we will suppose it loseth nothing of its swiftness. But the cause is, that their swiftness being equal, the one is longer in coming to the wall than the other, in proportion of time, as E G to E F. For though their swiftness be the same, considered in themselves, yet the swiftness of their approach to the wall is greater in E F than in E G, in proportion of the lines themselves.

A. When a bullet enters not, but rebounds from the wall, does it make the same angle going off, which it did falling on, as the sun-beams do?

B. If you measure the angles close by the wall their difference will not be sensible; otherwise it will be great enough, for the motion of the bullet grows continually weaker. But it is not so with the sun-beams which press continually and equally.

A. What is the cause of reflection? When a body can go no further on, it has lost its motion. Whence then comes the motion by which it reboundeth?

B. This motion of rebounding or reflecting proceedeth from the resistance. There is a difference to be considered between the reflection of light, and of a bullet, answerable to their different motions, pressing and striking. For the action which makes reflection of light, is the pressure of the air upon the reflecting body, caused by the sun, or other shining body, and is but a contrary endeavour; as if two men should press with their breasts

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upon the two ends of a staff, though they did not remove one another, yet they would find in themselves a great disposition to press backward upon whatsoever is behind them, though not a total going out of their places. Such is the way of reflecting light. Now, when the falling on of the sun-beams is oblique, the action of them is nevertheless perpendicular to the superficies it falls on. And therefore the reflecting body, by resisting, turneth back that motion perpendicularly, as from F to E; but taketh nothing from the force that goes on parallel in the line of E H, because the motion never presses. And thus of the two motions from F to E, and from E to H, is a compounded motion in the line F H, which maketh an angle in B G, equal to the angle F G E.

But in percussion (which is the motion of the bullet against a wall,) the bullet no sooner goeth off than it loseth of its swiftness, and inclineth to the earth by its weight. So that the angles made in falling on and going off, cannot be equal, unless they be measured close to the point where the stroke is made.

A. If a man set a board upright upon its edge, though it may very easily be cast down with a little pressure of one's finger, yet a bullet from a musket shall not throw it down, but go through it. What is the cause of that?

B. In pressing with your finger you spend time to throw it down. For the motion you give to the part you touch is communicated to every other part before it fall. For the whole cannot fall till every part be moved. But the stroke of a bullet is so swift, as it breaks through, before the motion

Problems of motion perpendicular, oblique, &c.

of the part it hits can be communicated to all the other parts that must fall with it.

A. The stroke of a hammer will drive a nail a great way into a piece of wood on a sudden. What weight laid upon the head of a nail, and in how much time will do the same? It is a question I have heard propounded amongst naturalists.

B. The different manner of the operation of weight from the operation of a stroke, makes it incalculable. The suddenness of the stroke upon one point of the wood takes away the time of resistance from the rest. Therefore the nail enters so far as it does. But the weight not only gives them time, but also augments the resistance; but how much, and in how much time, is, I think, impossible to determine.

A. What is the difference between reflection and recoiling?

B. Any reflection may, and not unproperly, be called recoiling; but not contrariwise every recoiling reflection. Reflection is always made by the reaction of a body pressed or stricken; but recoiling not always. The recoiling of a gun is not caused by its own pressing upon the gunpowder, but by the force of the powder itself, inflamed and moved every way alike.

A. I had thought it had been by the sudden reentering of the air after the flame and bullet were gone out. For it is impossible that so much room as is left empty by the discharging of the gun, should be so suddenly filled with the air that entereth at the touchhole.

B. The flame is nothing but the powder itself, which scattered into its smallest parts, seems of

Problems of motion perpendicular, oblique, &c.

greater bulk by much, than in truth it is, because they shine. And as the parts scatter more and more, so still more air gets between them, entering not only at the touchhole, but also at the mouth of the gun, which two ways being opposite, it will be much too weak to make the gun recoil.

A. I have heard that a great gun charged too much or tool ittle, will shoot, not above, nor below, but beside the mark; and charged with one certain charge between both, will hit it.

B. How that should be I cannot imagine. For when all things in the cause are equal, the effects cannot be unequal. As soon as fire is given, and before the bullet be out, the gun begins to recoil. If then there be any unevenness or rub in the ground more on one side than on the other, it shall shoot beside the mark, whether too much, or too little, or justly charged; because if the line wherein the gun recoileth decline, the way of the bullet will also decline to the contrary side of the mark. Therefore I can imagine no cause of this event, but either in the ground it recoils on, or in the unequal weight of the parts of the breech.

A. How comes refraction?

B. When the action is in a line perpendicular to the superficies of the body wrought upon, there will be no refraction at all. The action will proceed still in the same straight line, whether it be pression as in light, or percussion as in the shooting of a bullet. But when the pression is oblique, then will the refraction be that way which the nature of the bodies through which the action proceeds shall determine.

A. How is light refracted?

Problems of motion perpendicular, oblique, &c.

B. If it pass through a body of less, into a body of greater resistance, and to the point of the superficies it falleth on, you draw a line perpendicular to the same superficies, the action will proceed not in the same line by which it fell on, but in another line bending toward that perpendicular.

A. What is the reason of that?

B. I told you before, that the falling on worketh only in the perpendicular; but as soon as the action proceedeth farther inward than a mere touch, it worketh partly in the perpendicular, and partly forward, and would proceed in the same line in which it fell on, but for the greater resistance which now weakeneth the motion forward, and makes it to incline towards the perpendicular.

A. In transparent bodies it may be so; but there be bodies through which the light cannot pass at all.

B. But the action by which light is made, passeth through all bodies. For this action is pression; and whatsoever is pressed, presseth that which is next behind, and so continually. But the cause why there is no light seen through it, is the unevenness of the parts within, whereby the action is by an infinite number of reflections so diverted and weakened, that before it hath proceeded through, it hath not strength left to work upon the eye strongly enough to produce sight.

A. If the body being transparent, the action proceed quite through, into a body again of less resistance, as out of glass into the air, which way shall it then proceed in the air?

B. From the point where it goeth forth, draw a perpendicular to the superficies of the glass, the

Problems of motion perpendicular, oblique, &c.

action now freed from the resistance it suffered, will go from that perpendicular, as much as it did before come towards it.

A. When a bullet from out of the air entereth into a wall of earth, will that also be refracted towards the perpendicular.

B. If the earth be all of one kind, it will. For the parallel motion, will there also at the first entrance be resisted, which it was not before it entered.

A. How then comes a bullet, when shot very obliquely into any broad water, and having entered, yet to rise again into the air?

B. When a bullet is shot very obliquely, though the motion be never so swift, yet the approach downwards to the water is very slow, and when it cometh to it, it casteth up much water before it, which with its weight presseth downwards again, and maketh the water to rise under the bullet with force enough to master the weak motion of the bullet downwards, and to make it rise in such manner as bodies use to rise by reflection.

A. By what motion (seeing you ascribe all effects to motion) can a loadstone draw iron to it?

B. By the same motion hitherto supposed. But though all the smallest parts of the earth have this motion, yet it is not supposed that their motions are in equal circles; nor that they keep just time with one another; nor that they have all the same poles. If they had, all bodies would draw one another alike. For such an agreement of motion, of way, of swiftness, and of poles, cannot be maintained, without the conjunction of the bodies themselves in the centre of their common motion, but by violence. If therefore the iron have but so

Problems of motion perpendicular, oblique, &c.

much of the nature of the loadstone as readily to receive from it the like motion, as one string of a lute doth from another string strained to the same note, (as it is like enough it hath, the loadstone being but one kind of iron ore), it must needs after that motion received from it, unless the greatness of the weight hinder, come nearer to it, because at distance their motions will differ in time, and oppose each other, whereby they will be forced to a common centre. If the iron be lifted up from the earth, the motion of the loadstone must be stronger, or the body of it nearer, to overcome the weight; and then the iron will leap up to the loadstone as swiftly, as from the same distance it would fall down to the earth; but if both the stone and the iron be set floating upon the water, the attraction will begin to be manifest at a greater distance, because the hindrance of the weight is in part removed.

A. But why does the loadstone, if it float on a calm water, never fail to place itself at last in the meridian just north and south.

B. Not so, just in the meridian, but almost in all places with some variations. But the cause I think is, that the axis of this magnetical motion is parallel to the axis of the ecliptic, which is the axis of the like motion in the earth, and consequently that it cannot freely exercise its natural motion in any other situation.

A. Whence may this consent of motion in the loadstone and the earth proceed? Do you think, as some have written, that the earth is a great loadstone?

B. Dr. Gilbert, that was the first that wrote anything of this subject rationally, inclines to that opinion. Descartes thought the earth, excepting

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this upper crust of a few miles depth, to be of the same nature with all other stars, and bright. For my part, I am content to be ignorant; but I believe the loadstone hath been given its virtue by a long habitude in the mine, the vein of it lying in the plane of some of the meridians, or rather of some of the great circles that pass through the poles of the ecliptic, which are the same with the poles of the like motion supposed in the earth.

A. If that be true, I need not ask why the filings of iron laid on a loadstone equally distant from its poles will lie parallel to the axis, but on each side will incline to the pole that is next. Nor why by drawing a loadstone all along a needle of iron, the needle will receive the same poles. Nor why when the loadstone and iron, or two loadstones, are put together floating upon water, will fall one of them astern of the other, that their like parts may look the same way, and their unlike touch, in which action they are commonly said to repel one another. For all this may be derived from the union of their motions. One thing more I desire to know, and that is; what are those things they call spirits? I mean ghosts, fairies, hobgoblins, and the like apparitions.

B. They are no part of the subject of natural philosophy.

A. That which in all ages, and all places is commonly seen (as those have been, unless a great part of mankind be liars) cannot, I think, be supernatural.

B. All this that I have hitherto said, though upon better ground than can be had for a discourse of ghosts, you ought to take but for a dream.

A. I do so. But there be some dreams more

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like sense then others. And that which is like sense pleases me as well in natural philosophy, as if it were the very truth.

B. I was dreaming also once of these things; but was wakened by their noise. And they never came into any dream of mine since, unless apparitions in dreams and ghost be all one.

CHAPTER VIII.

THE DELPHIC PROBLEM, OR DUPLICATION OF THE CUBE.

A. HAVE you seen a printed paper sent from Paris, containing the duplication of the cube, written in French?

B. Yes. It was I that writ it, and sent it thither to be printed, on purpose to see what objections would be made to it by our professors of algebra here.

A. Then you have also seen the confutations of it by algebra.

B. I have seen some of them; and have one by me. For there was but one that was rightly calculated, and that is it which I have kept.

A. Your demonstration then is confuted though but by one.

B. That does not follow. For though an arithmetical calculation be true in numbers, yet the same may be, or rather must be false, if the units be not constantly the same.

A. Is their calculation so inconstant, or rather so foolish as you make it?

B. Yes. For the same number is sometimes so many lines, sometimes so many planes, and some-

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times so many solids; as you shall plainly see, if you will take the pains to examine first a demonstration I have to prove the said duplication, and after that, the algebraic calculation which is pretended to confute it. And not only that this one is false, but also any other arithmetical account used in geometry, unless the numbers be always so many lines, or always so many superficies, or always so many solids.

A. Let me see the geometrical demonstration.

B. There it is. Read it.

TO FIND A CUBE DOUBLE TO A CUBE GIVEN:

Let the side of the cube given be V D. Produce V D to A, till A D be double to D V. Then make the square of A D, namely A B C D. Divide A B and C D in the middle at E and F. Draw E F. Draw also A C cutting E F in I. Then in the sides B C and A D take B R and A S, each of them equal to A I or I C.

Lastly, divide S D in the middle at T, and upon the centre T, with the distance T V, describe a semi-circle cutting A D in Y, and D C in X.

I say the cube of D X is double to the cube of D V. For the three lines D Y, D X, D V are in continual proportion. And continuing the semicircle V X Y till it cut the line R S, drawn and produced in Z, the line S Z will be equal to D X. And drawing X Z it will pass through T. And the four lines T V, T X, T Y and T Z will be equal. And therefore joining Y X and Y Z, the figure V X Y Z will be a rectangle.

Produce C D to P so as D P be equal to A D. Now if Y Z produced fall on P, there will be three rectangle equiangled triangles, D P Y, D Y X, and

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D X V; and consequently four continual proportionals, D P, D Y, D X, and D V, whereof D X is the least of the means. And therefore the cube of D X will be double to the cube of D V.

A. That is true; and the cube of D Y will be double to the cube of D X; and the cube of D P double to the cube of D Y. But that Y Z produced, falls upon P, is the thing they deny, and which you ought to demonstrate.

B. If Y Z produced fall not on P, then draw P Y, and from V let fall a perpendicular upon P Y, suppose at *u*. Divide P V in the midst at *a*, and join *a u*; which done *a u* will be equal to *a* V or *a* P. For because V *u* P is a right angle, the point *u* will be in the semi-circle whereof P V is the diameter.

Therefore drawing V R, the angle *u* V R will be a right angle.

A. Why so?

B. Because T V and T Y are equal; and T D, T S equal; S Y will also be equal to D V. And because D P and R S are equal and parallel, R Y will be equal and parallel to P V. And therefore V R and P Y that join them will be equal and parallel. And the angles P *u* V, R V *u* will be alternate, and consequently equal. But P *u* V is a right angle; therefore also R V *u* will be a right angle.

A. Hitherto all is evident. Proceed.

B. From the point Y raise a perpendicular cutting V R wheresoever in *t*, and then (because P Y and V R are parallel) the angle Y *t* V will be a right angle. And the figure *u* Y *t* V a rectangle, and *u t* equal to Y V. But Y V is equal to Z X; and therefore Z X is equal to *u t*. And *u t* must pass through the point T (for the diameters of any

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rectangle divide each other in the middle), therefore Z and *u* are the same point, and X and *t* the same point. Therefore Y Z produced falls upon P. And D X is the lesser of the two means between A D and D V. And the cube of D X double to the cube of D V, which was to be demonstated.

A. I cannot imagine what fault there can be in this demonstration, and yet there is one thing which seems a little strange to me. And it is this. You take B R, which is half the diagonal, and which is the sine of forty-five degrees, and which is also the mean proportional between the two extremes; and yet you bring none of these proprieties into your demonstration. So that though you argue from the construction, yet you do not argue from the cause. And this perhaps your adversaries will object, at least, against the art of your demonstration, or enquire by what luck you pitched upon half the diagonal for your foundation.

B. I see you let nothing pass. But for answer you must know, that if a man argue from the negative of the truth, though he know not that it is the truth which is denied, yet he will fall at last, after many consequences, into one absurdity or another. For though false do often produce truth, yet it produces also absurdity, as it hath done here. But truth produceth nothing but truth. Therefore in demonstrations that tend to absurdity, it is no good logic to require all along the operation of the cause.

A. Have you drawn from hence no corollaries?

B. No. I leave that for others that will; unless you take this for a corollary, that, what arithmetical calculation soever contradicts it, is false.

A. Let me see now the algebraical demonstration against it.

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B. Here it is:

Let A B or A D be equal to . . .	2
Then D F or D V is equal to . . .	1
And B R or A S is equal to the square root of	2
And D Y equal to	3
want the square root of	2
The cube of A B is equal to . . .	8
The cube of D Y is equal to . . .	45
want the square root of 1682 that is almost equal to	4
For 45 want the square root of 1681 is equal to	4

Therefore D Y is a little less then the greater of the two means between A D and D V.

A. There is I see some little difference between this arithmetical and your geometrical demonstration. And though it be insensible, yet if his calculation be true, yours must needs be false, which I am sure cannot be.

B. His calculation is so true, that there is never a proposition in it false, till he come to the conclusion, that the cube of D Y is equal to 45, want the square root of 1682. But that, and the rest, is false.

A. I shall easily see that A D is certainly 2, whereof D V is 1, and A V is certainly 3, whereof D V is 1.

B. Right.

A. And B R is without doubt the square root of 2.

B. Why, what is 2?

A. 2 is the line A D as being double to D V which is 1.

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B. And so, the line B R is the square root of the line A D.

A. Out upon it, it is absurd. Why do you grant it to be true in arithmetic?

B. In arithmetic the numbers consist of so many units, and are never considered there as nothings. And therefore every one line has some latitude, and if you allow to B I, the semi-diagonal, the same latitude you do to A B, or to B R, you will quickly see the square of half the diagonal to be equal to twice the square of half A B.

A. Well, but then your demonstration is not confuted; for the point Y will have latitude enough to take in that little difference which is between the root of 1681 and the root of 1682. This putting off an unit sometimes for one line, sometimes for one square, must needs mar the reckoning. Again he says, the cube of A B is equal to 8; but seeing A B is 2, the cube of A B must be just equal to four of its own sides; so that the unit which was before sometimes a line, sometimes a square, is now a cube.

B. It can be no otherwise when you so apply arithmetic to geometry, as to number the lines of a plane, or the planes of a cube.

A. In the next place, I find that the cube of D Y is equal to 45, want the square root of 1682. What is that 45? Lines, or squares, or cubes?

B. Cubes; cubes of D V.

A. Then if you add to 45 cubes of D V the square root of 1682, the sum will be 45 cubes of D V; and if you add to the cube of D Y the same root of 1682, the sum will be the cube of D Y, plus the square root of 1682, and these two sums must be equal.

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B. They must so.

A. But the square root of 1682, being a line, adds nothing to a cube; therefore the cube alone of D Y, which he says is equal almost to 4 cubes of D V, is equal to 45 cubes of the same D V.

B. All these impossibilities do necessarily follow the confounding of arithmetic and geometry.

A. I pray you let me see the operation by which the cube of D Y (that is, the cube of 3, want the root of 2) is found equal to 45, want the square root of 1682.

B. Here it is.

A DETECTION OF THE ABSURD USE OF ARITHMETIC AS IT IS NOW APPLIED TO GEOMETRY.

$$\begin{array}{r} 3-\sqrt{2} \\ 3-\sqrt{2} \\ \hline -\sqrt{18}\quad+2 \\ 9-\sqrt{18}\qquad\; \\ \hline 9-\sqrt{72}\quad+2 \\ 3-\sqrt{2} \\ \hline -\sqrt{162}+12-\sqrt{8} \\ 27-\sqrt{648}+\;6\qquad\quad \\ \hline 27-\sqrt{658}-\sqrt{162}+18-\sqrt{8} \\ \hline\hline \end{array}$$

A. Why for two roots of 18 do you put the root of 72.

B. Because 2 roots of 18 are equal to one root of four times 18, which is 72

A. Next we have, that the root of 2 multiplied into 2 makes the root of 8. How is that true?

B. Does it not make 2 roots of two? And is not B R the root of 2, and 2 B R equal to the dia-

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gonal? And is not the square of the diagonal equal to 8 squares of D V?

A. It is true. But here the root of 8 is put for the cube of the root of 2. Can a line be equal to a cube?

B. No. But here we are in arithmetic again, and 8 is a cubic number.

A. How does the root of 2 multiplied into the root of 72 make 12?

B. Because it makes the root of 2 times 72, that is to say the root of 144 which is 12.

A. How does 9 roots of 2 make the root of 162?

B. Because it makes the root of 2 squares of 9, that is the root of 162.

A. How does 3 roots of 72 make the root of 648?

B. Because it makes the root of 9 times 72, that is of 648.

A. For the total sum I see 27 and 18, which make 45. Therefore the root of 648 together with the root of 162 and of 8, which are to be deducted from 45, ought to be equal to the root of 1682.

B. So they are. For 648 multiplied by 162
makes 104976, of which the double root is 648
and 648 and 162 added together make . . 810
Therefore the root of 648, added to the root
of 162, makes the root of . . . 1458
Again 1458 into 8 is 11664. The double
root whereof is 216
The sum of 1458 and 8 added together . 1466
The sum of 1466 and 216 is 1682, and the
root, the root of 1682

A. I see the calculation in numbers is right, though false in lines. The reason whereof can be no other than some difference between multiplying

The Delphic problem, or duplication of the cube.

numbers into lines or planes, and multipying lines into the same lines or planes.

B. The difference is manifest. For when you multiply a number into lines, the product is lines; as the number 2 multiplied into 3 lines is no more than 3 lines 2 times told. But if you multiply lines into lines you make planes, and if you multiply lines into planes you make solid bodies. In geometry there are but three dimensions, lengths, superficies, and body. In arithmetic there is but one, and that is number or length which you will. And though there be some numbers called plane, other solids, others plano-solid, others square, others cubic, others square-square, others quadrato-cubic, others cubi-cubic, &c., yet are all these but one dimension, namely number, or a file of things numbered.

A. But seeing this way of calculation by numbers is so apparently false, what is the reason this calculation came so near the truth?

B. It is because in arithmetic units are not nothings, and therefore have breadth. And therefore many lines set together make a superficies though their breadth be insensible. And the greater the number is into which you divide your line, the less sensible will be your error.

A. Archimedes, to find a straight line equal to the circumference of a circle, used this way of extracting roots. And it is the way also by which the table of sines, secants, and tangents have been calculated. Are they all out?

B. As for Archimedes, there is no man that does more admire him than I do: but there is no man that cannot err. His reasoning is good. But he, as all other geometricians before and after him, have had two principles that cross one another when they are applied to one and the same science. One is, that a point is no part of a line, which is true in geometry, where a part of a line when it is called a point, is not reckoned; another is, that a unit is part of a number; which is also true; but when they reckon by arithmetic in geometry, there a unit is sometimes part of a line, sometimes a part of a square, and sometimes part of a cube. As for the table of sines, secants, and tangents, I am not the first that find fault with them. Yet I deny not but they are true enough for the reckoning of acres in a map of land.

A. What a deal of labour has been lost by them that being professors of geometry have read nothing else to their auditors but such stuff as this you have here seen. And some of them have written great books of it in strange characters, such as in troublesome times, a man would suspect to be a cypher.

B. I think you have seen enough to satisfy you, that what I have written heretofore concerning the quadrature of the circle, and of other figures made in imitation of the parabola, has not been yet confuted.

A. I see you have wrested out of the hands of our antagonists this weapon of algebra, so as they can never make use of it again. Which I consider as a thing of much more consequence to the science of geometry, than either of the duplication of the cube, or the finding of two mean proportionals, or the quadrature of a circle, or all these problems put together.

FINIS.

DECAMERON PHYSIOLOGICUM;

OR,

TEN DIALOGUES OF NATURAL PHILOSOPHY.

BY

THOMAS HOBBES

OF MALMESBURY.

TO WHICH IS ADDED

THE PROPORTION OF A STRAIGHT LINE TO HALF THE ARC OF A QUADRANT,

BY THE SAME AUTHOR.

DECAMERON PHYSIOLOGICUM.

CHAPTER I.

OF THE ORIGINAL OF NATURAL PHILOSOPHY.

CHAP. I.

Of the original of natural philosophy.

A. I HAVE heard exceeding highly commended a kind of thing which I do not well understand, though it be much talked of, by such as have not otherwise much to do, by the name of philosophy; and the same again by others as much despised and derided: so that I cannot tell whether it be good or ill, nor what to make of it, though I see many other men that thrive by it.

B. I doubt not, but what so many do so highly praise must be very admirable, and what is derided and scorned by many, foolish and ridiculous. The honour and scorn falleth finally not upon philosophy, but upon the professors. Philosophy is *the knowledge of natural causes.* And there is no knowledge but of truth. And to know the true causes of things, was never in contempt, but in admiration. Scorn can never fasten upon truth. But the difference is all in the writers and teachers. Whereof some have neither studied, nor care for it, otherwise than as a trade to maintain themselves or gain preferment; and some for fashion, and to make themselves fit for ingenious company: and their study hath not been meditation, but acquiescence in the authority of those authors whom they

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have heard commended. And some, but few, there be, that have studied it for curiosity, and the delight which commonly men have in the acquisition of science, and in the mastery of difficult and subtil docrines. Of this last sort I count Aristotle, and a few others of the ancients, and some few moderns: and to these it is that properly belong the praises which are given to philosophy.

A. If I have a mind to study, for example natural philosophy, must I then needs read Aristotle, or some of those that now are in request?

B. There is no necessity of it. But if in your own meditation you light upon a difficulty, I think it is no loss of time, to enquire what other men say of it, but to rely only upon reason. For though there be some few effects of nature, especially concerning the heavens, whereof the philosophers of old time have assigned very rational causes, such as any man may acquiesce in, as of eclipses of the sun and moon by long observation, and by the calculation of their visible motions; yet what is that to the numberless and quotidian phenomena of nature? Who is there amongst them or their successors, that has satisfied you with the causes of gravity, heat, cold, light, sense, colour, noise, rain, snow, frost, winds, tides of the sea, and a thousand other things which a few men's lives are too short to go through, and which you and other curious spirits admire (as quotidian as they are), and fain would know the causes of them, but shall not find hem in the books of naturalists; and when you ask what are the causes of any of them, of a philo-opher now, he will put you off with mere words; h words, examined to the bottom, signify not

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a jot more than I cannot tell, or because it is: such as are intrinsical quality, occult quality, sympathy, antipathy, antiperistasis, and the like. Which pass well enough with those that care not much for such wisdom, though wise enough in their own ways; but will not pass with you that ask not simply what is the cause, but in what manner it comes about that such effects are produced.

A. That is cozening. What need had they of that? When began they thus to play the charlatans?

B. Need had they none. But know you not that men from their very birth, and naturally, scramble for every thing they covet, and would have all the world, if they could, to fear and obey them? If by fortune or industry one light upon a secret in nature, and thereby obtain the credit of an extraordinary knowing man, should he not make use of it to his own benefit? There is scarce one of a thousand but would live upon the charges of the people as far as he dares. What poor geometrician is there, but takes pride to be thought a conjurer? What mountebank would not make a living out of a false opinion that he were a great physician? And when many of them are once engaged in the maintenance of an error, they will join together for the saving of their authority to decry the truth.

A. I pray, tell me, if you can, how and where the study of philosophy first began.

B. If we may give credit to old histories, the first that studied any of the natural sciences were the astronomers of Ethiopia. My author is Diodorus Siculus, accounted a very faithful writer, who begins his history as high as is possible, and tells us that in Ethiopia were the first astronomers; and that for their predictions of eclipses, and other conjunctions and aspects of the planets, they obtained of their king not only towns and fields to a third part of the whole land, but were also in such veneration with the people, that they were thought to have discourse with their gods, which were the stars; and made their kings thereby to stand in awe of them, that they durst not either eat or drink but what and when they prescribed; no nor live, if they said the gods commanded them to die. And thus they continued in subjection to their false prophets, till by one of their kings, called Ergamenes, (about the time of the Ptolemies), they were put to the sword. But long before the time of Ergamenes, the race of these astrologers (for they had no disciples but their own children) was so numerous, that abundance of them (whether sent for or no I cannot tell) transplanted themselves into Egypt, and there also had their cities and lands allowed them, and were in request not only for astronomy and astrology, but also for geometry. And Egypt was then as it were an university to all the world, and thither went the curious Greeks, as Pythagoras, Plato, Thales, and others, to fetch philosophy into Greece. But long before that time, abundance of them went into Assyria, and had their towns and lands assigned them also there; and were by the Hebrews called Chaldees.

A. Why so?

B. I cannot tell; but I find in Martinius's Lexicon they are called Chasdim, and Chesdim, and (as he saith) from one Chesed the son of Nachor; but I

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find no such man as Chesed amongst the issue of Noah in the scripture. Nor do I find that there was any certain country called Chaldæa; though a town where any of them inhabited were called a town of the Chaldees. Martinius saith farther, that the same word Chasdim did signify also Demons.

A. By this reckoning I should conjecture they were called Chusdim, as being a race of Ethiopians. For the land of Chus is Ethiopia; and so the name degenerated first into Chuldim, and then into Chaldim; so that they were such another kind of people as we call gipsies; saving that they were admired and feared for their knavery, and the gipsies counted rogues.

B. Nay pray, except Claudius Ptolomæus, author of that great work of astronomy, the Almegest.

A. I grant he was excellent both in astronomy and geometry, and to be commended for his *Almegest;* but then for his *Judiciar Astrologie* annexed to it, he is again a gipsy. But the Greeks that travelled, you say, into Egypt, what philosophy did they carry home?

B. The mathematics and astronomy. But for that sublunary physics, which is commonly called natural philosophy, I have not read of any nation that studied it earlier than the Greeks, from whom it proceeded to the Romans. Yet both Greeks and Romans were more addicted to moral than to natural philosophy; in which kind we have their writings, but loosely and incoherently, written upon no other principles than their own passions and presumptions, without any respect to the laws of commonwealth, which are the ground and measure

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of all true morality. So that their books tend rather to teach men to censure than to obey the laws; which has been a great hindrance to the peace of the western world ever since. But they that seriously applied themselves to natural philosophy were but few, as Plato and Aristotle, whose works we have; and Epicurus whose doctrine we have in Lucretius. The writings of Philolaus and many other curious students being by fire or negligence now lost: though the doctrines of Philolaus concerning the motion of the earth have been revived by Copernicus, and explained and confirmed by Galileo now of late.

A. But methinks the natural philosophy of Plato, and Aristotle, and the rest, should have been cultivated and made to flourish by their disciples.

B. Whom do you mean, the successors of Plato, Epicurus, Aristotle, and the other first philosophers? It may be some of them may have been learned and worthy men. But not long after, and down to the time of our Saviour and his Apostles, they were for the most part a sort of needy, ignorant, impudent, cheating fellows, who by the profession of the doctrine of those first philosophers got their living. For at that time, the name of philosophy was so much in fashion and honour amongst great persons, that every rich man had a philosopher of one sect or another to be a schoolmaster to his children. And these were they that feigning Christianity, with their disputing and readiness of talking got themselves into Christian commons, and brought so many heresies into the primitive Church, every one retaining still a tang of what they had been used to teach.

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A. But those heresies were all condemned in the first Council of Nice.

B. Yes. But the Arian heresy for a long time flourished no less than the Roman, and was upheld by divers Emperors, and never fully extinguished as long as there were Vandals in Christendom. Besides, there arose daily other sects, opposing their philosophy to the doctrine of the Councils concerning the divinity of our Saviour; as how many persons he was, how many natures he had. And thus it continued till the time of Charlemagne, when he and Pope Leo the third divided the power of the empire into temporal and spiritual.

A. A very unequal division.

B. Why? Which of them think you had the greater share?

A. No doubt, the Emperor: for he only had the sword.

B. When the swords are in the hands of men, whether had you rather command the men or the swords?

A. I understand you. For he that hath the hands of the men, has also the use both of their swords and strength.

B. The empire thus divided into spiritual and temporal, the freedom of philosophy was to the power spiritual very dangerous. And for that cause it behoved the Pope to get schools set up not only for divinity, but also for other sciences, especially for natural philosophy. Which when by the power of the Emperor he had effected, out of the mixture of Aristotle's metaphysics with the Scripture, there arose a new science called School-divinity; which has been the principal learning of

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these western parts from the time of Charlemagne till of very late.

A. But I find not in any of the writings of the Schoolmen in what manner, from the causes they assign, the effect is naturally and necessarily produced.

B. You must not wonder at that. For you enquire not so much, when you see a change of anything, what may be said to be the cause of it, as how the same is generated; which generation is the entire progress of nature from the efficient cause to the effect produced. Which is always a hard question, and for the most part impossible for a man to answer to. For the alterations of the things we perceive by our five senses are made by the motion of bodies, for the most part, either for distance, smallness, or transparence, invisible.

A. But what need had they then to assign any cause at all, seeing that they could not show the effect was to follow from it?

B. The Schools, as I said, were erected by the Pope and Emperor, but directed by the Pope only, to answer and confute the heresies of the philosophers. Would you have them then betray their profession and authority, that is to say, their livelihood, by confessing their ignorance? Or rather uphold the same, by putting for causes, strange and unintelligible words; which might serve well enough not only to satisfy the people whom they relied on, but also to trouble the philosophers themselves to find a fault in.

A. Seeing you say that alteration is wrought by the motion of bodies, pray tell me first what I am to understand by the word body.

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B. It is a hard question, though most men think they can easily answer it, as that it is whatsoever they can see, feel, or take notice of by their senses. But if you will know indeed what is body, we must enquire first what there is that is not body. You have seen, I suppose, the effects of glasses, how they multiply and magnify the object of our sight; as when a glass of a certain figure will make a counter or a shilling seem twenty, though you be well assured there is but one. And if you set a mark upon it, you will find the mark upon them all. The counter is certainly one of those things we call bodies: are not the others so too?

A. No, without doubt. For looking through a glass cannot make them really more than they are.

B. What then be they but fancies, so many fancies of one and the same thing in several places?

A. It is manifest they are so many idols, mere nothings.

B. When you have looked upon a star or candle with both your eyes, but one of them a little turned awry with your finger, has not there appeared two stars, or two candles? And though you call it a deception of the sight, you cannot deny but there were two images of the object.

A. It is true, and observed by all men. And the same I say of our faces seen in looking-glasses, and of all dreams, and of all apparitions of dead men's ghosts; and wonder, since it is so manifest, I never thought upon it before, for it is a very happy encounter, and such as being by everybody well understood, would utterly destroy both idolatry and superstition, and defeat abundance of knaves that cheat and trouble the world with their devices.

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B. But you must not hence conclude that whosoever tells his dream, or sometimes takes his direction from it, is therefore an idolater, or superstitious, or a cheater. For God doth often admonish men by dreams of what they ought to do; yet men must be wary in this case that they trust not dreams with the conduct of their lives farther than by the laws of their country is allowed: for you know what God says, Deut. xiii: *If a prophet or a dreamer of dreams give thee a sign or a wonder, and the sign come to pass, yet if he bid thee serve other Gods let him be put to death.* Here by serving other Gods (since they have chosen God for their King) we are to understand revolting from their King, or disobeying of his laws. Otherwise I see no idolatry nor superstition in following a dream, as many of the Patriarchs in the Old Testament, and of the Saints in the New Testament did.

A. Yes: their own dreams. But when another man shall dream, or say that he has dreamed, and require me to follow that, he must pardon me if I ask him by what authority, especially if he look I should pay him for it.

B. But if commanded by the laws you live under, you ought to follow it. But when there proceed from one sound divers echoes, what are those echoes? And when with fingers crossed you touch a small bullet, and think it two; and when the me herb or flower smells well to one and ill to other, and the same at several times, well and ill to rself, and the like of tastes, what are those oes, feelings, odours, and tastes?

. It is manifest they are all but fancies. But inly when the sun seems to my eye no bigger

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than a dish, there is behind it somewhere somewhat else, I suppose a real sun, which creates those fancies, by working, one way or other, upon my eyes, and other organs of my senses, to cause that diversity of fancy.

B. You say right; and that is it I mean by the word body, which briefly I define to be any thing that hath a being in itself, without the help of sense.

A. Aristotle, I think, meaneth by body, ***substance***, or ***subjectum***, wherein colour, sound, and other fancies are, as he says, inherent. For the word essence has no affinity with substance. And Seneca says, he understands it not. And no wonder: for essence is no part of the language of mankind, but a word devised by philosophers out of the copulation of two names, as if a man having two hounds could make a third, if it were need, of their couples.

B. It is just so. For having said in themselves, (for example): *a tree is a plant*, and conceiving well enough what is the signification of those names, knew not what to make of the word *is*, that couples those names; nor daring to call it a body, they called it by a new name (derived from the word ***est***), ***essentia***, and ***substantia***, deceived by the idiom of their own language. For in many other tongues, and namely in the Hebrew, there is no such copulative. They thought the names of things sufficiently connected, when they are placed in their natural consequence; and were therefore never troubled with essences, nor other fallacy from the copulative ***est***.

CHAPTER II.

OF THE PRINCIPLES AND METHOD OF NATURAL PHILOSOPHY.

CHAP. II.

Of the principles and method of natural philosophy.

A. THIS history of the old philosophers has not put me out of love, but out of hope of philosophy from any of their writings. I would therefore try if I could attain any knowledge therein by my own meditation: but I know neither where to begin, nor which way to proceed.

B. Your desire, you say, is to know the causes of the effects or phenomena of nature; and you confess they are fancies, and, consequently, that they are in yourself; so that the causes you seek for only are without you, and now you would know how those external bodies work upon you to produce those phenomena. The beginning therefore of your enquiry ought to be at; *What it is you call a cause?* I mean an efficient cause: for the philosophers make four kinds of causes, whereof the efficient is one. Another they call the formal cause, or simply the form or essence of the thing caused; as when they say, four equal angles and four equal sides are the cause of a square figure; or that heaviness is the cause that makes heavy
to descend; but that is not the cause you
, nor any thing but this: *It descends be-*
descends. The third is the material cause,
they say, the walls and roof, &c. of a house
use of a house. The fourth is the final
hath place only in moral philosophy.

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A. We will think of final causes upon some other occasion; of formal and material not at all: I seek only the efficient, and how it acteth from the beginning to the production of the effect.

B. I say then, that in the first place you are to enquire diligently into the nature of motion. For the variations of fancies, or (which is the same thing) of the phenomena of nature, have all of them one universal efficient cause, namely the variety of motion. For if all things in the world were absolutely at rest, there could be no variety of fancy; but living creatures would be without sense of all objects, which is little less than to be dead.

A. What if a child new taken from the womb should with open eyes be exposed to the azure sky, do not you think it would have some sense of the light, but that all would seem unto him darkness?

B. Truly, if he had no memory of any thing formerly seen, or by any other sense perceived, (which is my supposition), I think he would be in the dark. For darkness is darkness, whether it be black or blue, to him that cannot distinguish.

A. Howsoever that be, it is evident enough that whatsoever worketh is moved: for action is motion.

B. Having well considered the nature of motion, you must thence take your principles for the foundation and beginning of your enquiry.

A. As how?

B. Explain as fully and as briefly as you can what you constantly mean by motion; which will save yourself as well as others from being seduced by equivocation.

A. Then I say, motion is nothing but change of

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place for all the effect of a body upon the organs of our senses is nothing but fancy. Therefore we can fancy nothing from seeing it moved, but change of place.

B. It is right. But you must then tell me also what you understand by place: for all men are not yet agreed on that.

A. Well then; seeing we fancy a body, we cannot but fancy it somewhere. And therefore I think place is the fancy of here or there.

B. That is not enough. Here and there are not understood by any but yourself, except you point towards it. But pointing is no part of a definition. Besides, though it help him to find the place, it will never bring him to it.

A. But seeing sense is fancy, when we fancy a body, we fancy also the figure of it, and the space it fills up. And then I may define place to be the precise space within which the body is contained. For space is also part of the image we have of the object seen.

B. And how define you time?

A. As place is to a body, so, I think, is time to the motion of it; and consequently I take time to be our fancy or image of the motion. But is there any necessity of so much niceness?

B. Yes. The want of it is the greatest, if not the only, cause of all the discord amongst philosophers, as may easily be perceived by their abusing and confounding the names of things that differ in their nature; as you shall see when there is occasion to recite some of the tenets of divers philosophers.

A. I will avoid equivocation as much as I can.

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And for the nature of motion, I suppose I understand it by the definition. What is next to be done?

B. You are to draw from these definitions, and from whatsoever truth else you know by the light of nature, such general consequences as may serve for axioms, or principles of your ratiocination.

A. That is hard to do.

B. I will draw them myself, as many as for our present discourse of natural causes we shall have need of; so that your part will be no more than to take heed I do not deceive you.

A. I will look to that.

B. My first axiom then shall be this: Two bodies, at the same time, cannot be in one place.

A. That is true: for we number bodies as we fancy them distinct, and distinguish them by their places. You may therefore add: nor one body at the same time in two places. And philosophers mean the same, when they say: there is no penetration of bodies.

B. But they understand not their own words: for penetration signifies it not. My second axiom is, that nothing can begin, change, or put an end to, its own motion. For supposing it begin just now, or being now in motion, change its way or stop; I require the cause why now rather than before or after, having all that is necessary to such motion, change, or rest, alike at all times?

A. I do not doubt but the argument is good in bodies inanimate; but perhaps in voluntary agents it does not hold.

B. How it holds in voluntary agents we will then consider when our method hath brought us to

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the powers and passions of the mind. A third axiom shall be this: whatsoever body being at rest is afterwards moved, hath for its immediate movement some other body which is in motion and toucheth it. For, since nothing can move itself, the movent must be external. And because motion is change of place, the movent must put it from its place, which it cannot do till it touch it.

A. That is manifest, and that it must more than touch it; it must also follow it. And if more parts of the body are moved than are by the movent touched, the movent is not immediate. And by this reason, a continued body, though never so great, if the first superficies be pressed never so little back, the motion will proceed through it.

B. Do you think that to be impossible? I will prove it from your own words: for you say that the movent does then touch the body which it moveth. Therefore it puts it back; but that which is put back, puts back the next behind, and that again the next; and so onward to any distance, the body being continued. The same is also manifest by experience, seeing one that walks with a staff can distinguish, though blind, between stone and glass; which were impossible, if the parts of his staff between the ground and his hand made no resistance. So also he that in the silence of the night lays his ear to the ground, shall hear the treading of men's feet farther than if he stood upright.

A. This is certainly true of a staff or other hard body, because it keeps the motion in a straight line from diffusion. But in such a fluid body as the air, which being put back must fill an orb, and

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the farther it is put back, the greater orb, the motion will decrease, and in time, by the resistance of air to air, come to an end.

B. That any body in the world is absolutely at rest, I think not true: but I grant, that in a space filled everywhere with body, though never so fluid, if you give motion to any part thereof, that motion will by resistance of the parts moved, grow less and less, and at last cease; but if you suppose the space utterly void, and nothing in it, then whatsoever is once moved shall go on eternally: or else that which you have granted is not true, viz., that nothing can put an end to its own motion.

A. But what mean you by resistance?

B. Resistance is the motion of a body in a way wholly or partly contrary to the way of its movent, and thereby repelling or retarding it. As when a man runs swiftly, he shall feel the motion of the air in his face. But when two hard bodies meet, much more may you see how they abate each other's motion, and rebound from one another. For in a space already full, the motion cannot, in an instant, be communicated through the whole depth of the body that is to be moved.

A. What other definitions have I need of?

B. In all motion, as in all quantity, you must take the beginning of your reckoning from the least supposed motion. And this I call the first endeavour of the movent; which endeavour, how weak soever, is also motion. For if it have no effect at all, neither will it do anything though doubled, trebled, or by what number soever multiplied: for nothing, though multiplied, is still nothing. Other

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axioms and definitions we will take in, as we need them, by the way.

A. Is this all the preparation I am to make?

B. No, you are to consider also the several kinds and properties of motion, viz., when a body being moved by one or more movents at once, in what way it is carried, straight, circular, or otherwise crooked; and what degree of swiftness; as also the action of the movent, whether trusion, vection, percussion, reflection, or refraction; and farther you must furnish yourself with as many experiments (which they call phenomenon) as you can. And supposing some motion for the cause of your phenomenon, try, if by evident consequence, without contradiction to any other manifest truth or experiment, you can derive the cause you seek for from your supposition. If you can, it is all that is expected, as to that one question, from philosophy. For there is no effect in nature which the Author of nature cannot bring to pass by more ways than one.

A. What I want of experiments you may supply out of your own store, or such natural history as you know to be true; though I can be well content with the knowledge of the causes of those things which everybody sees commonly produced. Let us therefore now enquire the cause of some ect particular.

We will begin with that which is the most rsal, the universe; and enquire in the first if any place be absolutely empty, that is to the language of philosophers, whether there vacuum in nature?

CHAPTER III.

OF VACUUM.

CHAP. III. Of vacuum.

A. It is hard to suppose, and harder to believe, that the infinite and omnipotent Creator of all things should make a work so vast as is the world we see, and not leave a few little spaces with nothing at all in them; which put altogether in respect of the whole creation, would be insensible.

B. Why say you that? Do you think any argument can be drawn from it to prove there is vacuum?

A. Why not? For in so great an agitation of natural bodies, may not some small parts of them be cast out, and leave the places empty from whence they were thrown?

B. Because He that created them is not a fancy, but the most real substance that is; who being infinite, there can be no place empty where He is, nor full where He is not.

A. It is hard to answer this argument, because I do not remember that there is any argument for the maintenance of vacuum in the writings of divines: therefore I will quit that argument, and come to another. If you take a glass vial with a narrow neck, and having sucked it, dip it presently at the neck into a basin of water, you shall manifestly see the water rise into the vial. Is not this a certain sign that you had sucked out some of the air, and consequently that some part of the vial was left empty?

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B. No; for when I am about to suck, and have air in my mouth, contracting my checks I drive the same against the air in the glass, and thereby against every part of the sides of the hard glass. And this gives to the air within an endeavour outward, by which, if it be presently dipped into the water, it will penetrate and enter into it. For air if it be pressed will enter into any fluid, much more into water. Therefore there shall rise into the vial so much water as there was air forced into the basin.

A. This I confess is possible, and not improbable.

B. If sucking would make vacuum, what would become of those women that are nurses? Should they not be in a very few days exhausted, were it not that either the air which is in the child's mouth penetrateth the milk as it descends, and passeth through it, or the breast is contracted?

A. From what experiment can you evidently infer that there is no vacuum?

B. From many, and such as to almost all men are known and familiar. If two hard bodies, flat and smooth, be joined together in a common superficies parallel to the horizontal plane, you cannot without great force pull them asunder, if you apply your force perpendicularly to the common superficies: but if you place that common superficies erect to the horizon, they will fall asunder with their own weight. From whence I argue thus: since their contiguity, in what posture soever, is the same, and that they cannot be pulled asunder by a perpendicular force without letting in the ambient air in an instant, which is impossible; or almost in an instant, which is difficult: and on the

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other side, when the common superficies is erect, the weight of the same hard bodies is able to break the contiguity, and let in the air successively; it is manifest that the difficulty of separation proceeds from this, that neither air nor any other body can be moved to any, how small soever, distance in an instant; but may easily be moved (the hardness at the sides once mastered) successively. So that the cause of this difficulty of separation is this, that they cannot be parted except the air or other matter can enter and fill the space made by their diremption. And if they were infinitely hard, not at all. And hence also you may understand the cause why any hard body, when it is suddenly broken, is heard to crack; which is the swift motion of the air to fill the space beween. Another experiment, and commonly known, is of a barrel of liquor, whose taphole is very little, and the bung so stopped as to admit no air; for then the liquor will not run: but if the tap-hole be large it will, because the air pressed by a heavier body will pierce through it into the barrel. The like reason holds of a gardener's watering-pot, when the holes in the bottom are not too great. A third experiment is this: turn a thin brass kettle the bottom upwards, and lay it flat upon the water. It will sink till the water rise within to a certain height, but no higher: yet let the bottom be perforated, and the kettle will be full and sink, and the air rise again through the water without. But if a bell were so laid on, it would be filled and sink, though it were not perforated, because the weight is greater than the weight of the same bulk of water.

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A. By these experiments, without any more, I am convinced, that there is not actually in nature any vacuum; but I am not sure but that there may be made some little place empty, and this from two experiments, one whereof is Toricellius' experiment, which is this: take a cylinder of glass, hollow throughout, but close at the end, in form of a sack.

B. How long?

A. As long as you will, so it be more than twenty-nine inches.

B. And how broad?

A. As broad as you will, so it be broad enough to pour into it quicksilver. And fill it with quicksilver, and stop up the entrance with your finger, so as to unstop it again at your pleasure. Then set down a basin, or, if you will, a sea of quicksilver, and inverting the cylinder full as it is, dip the end into the quicksilver, and remove your finger, that the cylinder may empt itself. Do you conceive me? For there is so many passing by, that I cannot paint it.

B. Yes, I conceive you well enough. What follows?

A. The quicksilver will descend in the cylinder, not till it be level with that in the basin, according to the nature of heavy fluids, but stay and stand above it, at the height of twenty-nine inches or very near it, the bottom being now uppermost, that no air can get in.

B. What do you infer from this?

A. That all the cavity above twenty-nine inches is filled with vacuum.

B. It is very strange that I, from this same experiment, should infer, and I think evidently, that it is filled with air. I pray, tell me, when you had

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inverted the cylinder, full as it was, and stopped with your finger, dipped into the basin, if you had then removed your finger, whether you think the quicksilver would not all have fallen out?

A. No sure. The air would have been pressed upward through the quicksilver itself: for a man with his hand can easily thrust a bladder of air to the bottom of a basin of quicksilver.

B. It is therefore manifest that quicksilver can press the air through the same quicksilver.

A. It is manifest; and also itself rise into the air.

B. What cause then can there be, why it should stand still at twenty nine inches above the level of the basin, rather than any place else?

A. It is not hard to assign the cause of that. For so much quicksilver as was above the twenty-nine inches, will rise the first level of that in the basin, as much as if you had poured it on; and thereby bring it to an equilibrium. So that I see plainly now, that there is no necessity of vacuum from this experiment. For I considered only that naturally quicksilver cannot ascend in air, nor air descend in quicksilver, though by force it may.

B. Nor do I think that Torricellius or any other vacuist thought of it more than you. But what is the second experiment?

A. There is a sphere of glass, which they call a recipient, of the capacity of three or four gallons. And there is inserted into it the end of a hollow cylinder of brass above a foot long; so that the whole is one vessel, and the bore of the cylinder three inches diameter. Into which is thrust by force a solid cylinder of wood, covered with leather so just, as it may in every point exactly touch

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the concave superficies of the brass. There is also, to let out the air which the wooden cylinder as it enters (called the sucker) drives before it, a flap to keep out the external air while they are pulling the sucker. Besides, at the top of the recipient there is a hole to put into it anything for experiment. The sucker being now forced up into the cylinder, what do you think must follow?

B. I think it will require as much strength to pull it back, as it did to force it in.

A. That is not it I ask, but what would happen to the recipient?

B. I think so much air as would fill the place the sucker leaves, would descend into it out of the recipient; and also that just so much from the external air would enter into the recipient, between the brass and the wood, at first very swiftly, but, as the place increased, more leisurely.

A. Why may not so much air rather descend into the place forsaken, and leave as much vacuum as that comes to in the recipient? For otherwise no air will be pumped out, nor can that wooden pestle be called a sucker.

B. That is it I say. There is no air either pumped or sucked out.

A. How can the air pass between the leather and the brass, or between the leather and the wood, being so exactly contiguous, or through the
r itself?

onceive no such exact contiguity, nor such
of the leather: for I never yet had any
storm would keep out either air or water.
how then could there be made in the

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recipient such strange alteration both on animate and inanimate bodies?

B. I will tell you how. The air descends out of the recipient, because the air which the sucker removeth from behind itself, as it is pulling out, has no place to retire into without, and therefore is driven into the engine between the wood of the sucker and the brass of the cylinder, and causes as much air to come into the place forsaken by the retiring sucker; which causeth, by oft repetition of the force, a violent circulation of the air within the recipient, which is able quickly to kill anything that lives by respiration, and make all the alterations that have appeared in the engine.

CHAPTER IV.

OF THE SYSTEM OF THE WORLD.

B. You are come in good time; let us therefore sit down. There is ink, paper, ruler, and compass. Draw a little circle to represent the body of the sun.

A. It is done. The centre is A, the circumference is L M.

B. Upon the same centre A, draw a larger circle to stand for the ecliptic: for you know the sun is always in the plane of the ecliptic.

A. There it is. The diameters of it at right angles are B Z.

B. Draw the diameter of the equator.

A. How?

B. Through the centre A (for the earth is also always in the plane of the equator or of some of its

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parallels) so as to be distant from B twenty-three degrees and a half.

A. Let it be H I: and let C G be equal to B H; and so C will be one of the poles of the ecliptic, suppose the north-pole; and than H will be east, and I west. And C A produced to the circumference in E, makes E the south-pole.

B. Take C K equal to C G, and the chord G K will be the diameter of the arctic circle, and parallel to H I, the diameter of the equator. Lastly, upon the point B, draw a little circle wherein I suppose to be the globe of the earth.

A. It is drawn, and marked with *l m.* And B D and K G joined will be parallel; and as H and I are east and west, and so are B and D, and G and K.

B. True; but producing Z B to the circumference *l m* in *b*, the line B *b* will be in the diameter of the ecliptic of the earth, and B *m* in the diameter of the equator of the earth. In like manner, if you produce K G cutting the circle, whose centre is G, in *d* and *e*, and make an angle *n* G *d* equal to *b* B *m*, the line *n* G will be in the ecliptic of the earth, because G *d* is in the equator of the earth. So that in the annual motion of the earth through the ecliptic, every straight line drawn in the earth, is perpetually kept parallel to the place from whence it is removed.

A. It is true; and it is the doctrine of Copernicus. But I cannot yet conceive by what one motion this circle can be described otherwise than we are taught by Euclid. And then I am sure that all the diameters shall cross one another in the centre, which in this figure is A.

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B. I do not say that the diameters of a sphere or circle can be parallel; but that if a circle of a lesser sphere be moved upon the circumference of a great circle of a greater sphere, that the straight lines that are in the lesser sphere may be kept parallel perpetually to the places they proceed from.

A. How? And by what motion?

B. Take into your hand any straight line (as in this figure), the line L A M, which we suppose to be the diameter of the sun's body; and moving it parallelly with the ends in the circumference, so as that the end M may withal describe a small circle, as M *a*. It is manifest that all the other points of the same line L M will, by the same motion, at the same time, describe equal circles to it. Likewise if you take in your hand any two diameters fastened together, the same parallel motion of the line L M, shall cause all the points of the other diameter to make equal circles to the same M *a*.

A. It is evident; as also that every point of the sun's body shall do the like. And not only so, but also if one end describe any other figure, all the other points of the body shall describe like and equal figures to it.

B. You see by this, that this parallel motion is compounded of two motions, one circular upon the superficies of a sphere, the other a straight motion from the centre to every point of the same superficies, and beyond it.

A. I see it.

B. It follows hence, that the sun by this motion must every way repel the air; and since there is no empty place for retiring, the air must turn about in a circular stream; but slower or swifter according

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as it is more or less remote from the sun; and that according to the nature of fluids, the particles of the air must continually change place with one another; and also that the stream of the air shall be the contrary way to that of the motion, for else the air cannot be repelled.

A. All this is certain.

B. Well; then if you suppose the globe of the earth to be in this stream which is made by the motion of the sun's body from east to west, the stream of air wherein is the earth's annual motion will be from west to east.

A. It is certain.

B. Well. Then if you suppose the globe of the earth, whose circle is moved annually, to be *l m*, the stream of the air without the ecliptic falling upon the superficies of the earth *l m* without the ecliptic, being slower, and the stream that falleth within swifter, the earth shall be turned upon its own centre proportionally to the greatness of the circles; and consequently their diameters shall be parallel; as also are other straight lines correspondent.

A. I deny not but the streams are as you say; and confess that the proportion of the swiftness without, is to the swiftness within, as the sun's ecliptic to the ecliptic of the earth; that is to say, as the angle H A B to the angle *m* B *b*. And I like your argument the better, because it is drawn from Copernicus his foundation. I mean the compounded motion of straight and circular.

B. I think I shall not offer you many demonstrations of physical conclusions that are not derived from the motions supposed or proved by

Copernicus. For those conclusions in natural philosophy I most suspect of falshood, which require most variety of suppositions for their demonstrations.

A. The next thing I would know, is how great or little you suppose that circle *a* M?

B. I suppose it less than you can make it: for there appears in the sun no such motion sensible. It is the first endeavour of the sun's motion. But for all that, as small as the circle is, the motion may be as swift, and of as great strength as it is possible to be named. It is but a kind of trembling that necessarily happeneth in those bodies, which with great resistance press upon one another.

A. I understand now from what cause proceedeth the annual motion. Is the sun the cause also of the diurnal motion?

B. Not the immediate cause. For the diurnal motion of the earth is upon its own centre, and therefore the sun's motion cannot describe it. But it proceedeth as a necessary consequence from the annual motion. For which I have both experience and demonstration. The experiment is this: into a large hemisphere of wood, spherically concave, put in a globe of lead, and with your hands hold it fast by the brim, moving your hand circularly, but in a very small compass; you shall see the globe circulate about the concave vessel, just in the same manner as the earth doth every year in the air; and you shall see withal, that as it goes, it turns perpetually upon its own centre, and very swiftly.

A. I have seen it: and it is used in some great kitchens to grind mustard.

B. Is it so? Therefore take a hemisphere of

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gold, if you have it, the greater the better, and a bullet of gold, and, without mustard, you shall see the same effect.

A. I doubt it not. But the cause of it is evident. For any spherical body being in motion upon the sides of a concave and hard sphere, is all the way turned upon its own centre by the resistance of the hard wood or metal. But the earth is a bullet without weight, and meeteth only with air, without any harder body in the way to resist it.

B. Do you think the air makes no resistance, especially to so swift a motion as is the annual motion of the earth? If it do make any resistance, you cannot doubt but that it shall turn the earth circularly, and in a contrary way to its annual motion; that is to say, from east to west, because the annual motion is from west to east.

A. I confess it. But what deduce you from these motions of the sun?

B. I deduce, first, that the air must of necessity be moved both circularly about the body of the sun according to the ecliptic, and also every way directly from it. For the motion of the sun's body is compounded of this circular motion upon the sphere L M, and of the straight motion of its semi-diameters from the centre A to the superficies of the sun's body, which is L M. And therefore the air must needs be repelled every way, and also continually change place to fill up the places for-ken by other parts of the air, which else would empty, there being no vacuum to retire unto. that there would be a perpetual stream of air, in a contrary way to the motion of the sun's

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body, such as is the motion of water by the sides of a ship under sail.

A. But this motion of the earth from west to east is only circular, such as is described by a compass about a centre; and cannot therefore repel the air as the sun does. And the disciples of Copernicus will have it to be the cause of the moon's monthly motion about the earth.

B. And I think Copernicus himself would have said the same, if his purpose had been to have shown the natural causes of the motions of the stars. But that was no part of his design; which was only from his own observations, and those of former astronomers, to compute the times of their motions; partly to foretel the conjunctions, oppositions, and other aspects of the planets; and partly to regulate the times of the Church's festivals. But his followers, Kepler and Galileo, make the earth's motion to be the efficient cause of the monthly motion of the moon about the earth; which without the like motion to that of the sun in L M, is impossible. Let us therefore for the present take it in as a necessary hypothesis; which from some experiment that I shall produce in our following discourses, may prove to be a certain truth.

A. But seeing A is the centre both of the sun's body and of the annual motion of the earth, how can it be (as all astronomers say it is) that the orb of the annual motion of the earth should be eccentric to the sun's body? For you know that from the vernal equinox to the autumnal, there be one hundred and eighty-seven days; but from the autumnal equinox to the vernal, there be but one

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hundred and seventy-eight days. What natural cause can you assign for this eccentricity?

B. Kepler ascribes it to a magnetic virtue, viz. that one part of the earth's superficies has a greater kindness for the sun than the other part.

A. I am not satisfied with that. It is magical rather than natural, and unworthy of Kepler. Tell me your own opinion of it.

B. I think that the magnetical virtue he speaks of, consisteth in this: that the southern hemisphere of the earth is for the greatest part sea, and that the greatest part of the northern hemisphere is dry land. But how it is possible that from thence should proceed the eccentricity (the sun being nearest to the earth, when he is in the winter solstice), I shall show you when we come to speak of the motions of air and water.

A. That is time enough: for I intend it for our next meeting. In the mean time I pray you tell me what you think to be the cause why the equinoctial, and consequently the solstitial, points are not always in one and the same point of the ecliptic of the fixed stars. I know they are not, because the sun does not rise and set in points diametrically opposite: for if it did, there would be no difference of the seasons of the year.

B. The cause of that can be no other, than that the earth, which is *l m*, hath the like motion to that which I suppose the sun to have in L M, compounded of straight and circular from west to east in a day, as the annual motion hath in a year; so that, not reckoning the eccentricity, it will be moved through the ecliptics in one revolution, as Copernicus proveth, about one degree. Suppose

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then the whole earth moved from H to I, (which is half the year) circularly, but falling from I to *i* in the same time about thirty minutes, and as much in the other hemisphere from H to *k*; then draw the line *i k*, which will be equal and parallel to H I, and be the diameter of the equator for the next year. But it shall not cut the diameter of the ecliptic B Z in A, which was the equinoctial of the former year, but in *o* thirty-six seconds from the first degree of Aries. Suppose the same done in the hemisphere under the plane of the paper, and so you have the double of thirty-six seconds, that is seventy-two seconds, or very near, for the progress of the vernal equinox in a year. The cause why I suppose the arch I *i* to be half a degree in the ecliptic of the earth, is, that Copernicus and other astronomers, and experience, agree in this, that the equinoctial points proceed according to the order of the signs, Aries, Taurus, Gemini, &c. from west to east every hundredth year one degree or very near.

A. In what time do they make the whole revolution through the ecliptic of the sky?

B. That you may reckon. For we know by experience that it hath proceeded about one degree, that is sixty minutes, constantly a long time in a hundred years. But as one hundred years to one degree, so is thirty-six thousand years to three hundred and sixty degrees. Also as one hundred years to one degree, so is one year to the hundredth part of one degree, or sixty minutes; which is $\frac{60}{100}$, or thirty-six seconds for the progress of one year; which must be somewhat more than a degree according to Copernicus, who, (lib. iii. cap. 2) saith, AP. IV.

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that for four hundred years before Ptolomy it was one degree almost constantly. Which is well enough as to the natural cause of the precession of the equinoctial points, which is the often-said compounded motion, though not an exact astronomical calculation.

A. And it is a great sign that his supposition is true. But what is the cause that the obliquity of the ecliptic, that is, the distance between the equinoctial and the solstice, is not always the same?

B. The necessity of the obliquity of the ecliptic is but a consequence to the precession of the equinoctial points. And therefore, if from C, the north pole, you make a little circle, C *u*, equal to fifteen minutes of a degree upon the earth, and another, *u s*, equal to the same, which will appear like this figure 8, that is, (as Copernicus calls it), a circle twined, the pole C will be moved half the time of the equinoctial points, in the arc C *u*, and as much in the alternate arc *u s* descending to *s*. But in the arc *s u*, and its alternate rising to C, the cause of the twining is the earth's annual motion the same way in the ecliptic, and makes the four quarters of it; and makes also their revolution twice as slow as that of the equinoctial points. And, therefore, the motion of it is the same compounded motion which Copernicus takes for his supposition, and is the cause of the precession of the equinoctial points, and consequently of the variation of the obliquity, adding to it or taking from it somewhere more, somewhere less; so as that one with another the addition is not much more, nor the subtraction much less than

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thirty minutes. But as for the natural efficient cause of this compounded motion, either in the sun, or the earth, or any other natural body, it can be none but the immediate hand of the Creator.

A. By this it seems that the poles of the earth are always the same, but make this 8 in the sphere of the fixed stars near that which is called Cynosura.

B. No: it is described on the earth, but the annual motion describes a circle in the sphere of the fixed stars. Though I think it improper to say a sphere of the fixed stars, when it is so unlikely that all the fixed stars should be in the superficies of one and the same globe.

A. I do not believe they are.

B. Nor I, since they may seem less one than another, as well by their different distances, as by their different magnitudes. Nor is it likely that the sun (which is a fixed star) is the efficient cause of the motion of those remoter planets, Mars, Jupiter, and Saturn; seeing the whole sphere, whose diameter is the distance between the sun and the earth, is but a point in respect of the distance between the sun and any other fixed star. Which I say only to excite those that value the knowledge of the cause of comets, to look for it in the dominion of some other sun than that which moveth the earth. For why may not there be some other fixed star, nearer to some planet than is the sun, and cause such a light in it as we call a comet?

A. As how?

B. You have seen how in high and thin clouds above the earth, the sun-beams piercing them have

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appeared like a beard; and why might not such a beard have appeared to you like a comet, if you had looked upon it from as high as some of the fixed stars?

A. But because it is a thing impossible for me to know, I will proceed in my own way of inquiry. And seeing you ascribe this compounded motion to the sun and earth, I would grant you that the earth (whose annual motion is from west to east) shall give the moon her monthly motion from east to west. But then I ask you whether the moon have also that compounded motion of the earth, and with it a motion upon its own centre, as hath the earth? For seeing the moon has no other planet to carry about her, she needs it not.

B. I see reason enough, and some necessity, that the moon should have both those motions. For you cannot think that the Creator of the stars, when he gave them their circular motion, did first take a centre, and then describe a circle with a chain or compass, as men do? No; he moved all the parts of a star together and equally in the creation: and that is the reason I give you. The necessity of it comes from this phenomenon, that the moon doth turn one and the same face towards the earth; which cannot be by being moved about the earth parallelly, unless also it turn about its own centre. Besides, we know by experience, that the motion of the moon doth add not a little to the motion of the sea: which were impossible if it did not add to the stream of the air, and by consequence to that of the water.

A. If you could get a piece of the true and intimate substance of the earth, of the bigness of

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a musket-bullet, do you believe that the bullet would have the like compounded motion to that which you attribute to the sun, earth, and moon?

B. Yes, truly; but with less strength, according to its magnitude; saving that by its gravity falling to the earth, the activity of it would be unperceived.

A. I will trouble you no more with the nature of celestial appearances; but I pray you tell me by what art a man may find what part of a circle the diameter of the sun's body doth subtend in the ecliptic circle?

B. Kepler says it subtends thirty minutes, which is half a degree. His way to find it is by letting in the sun-beams into a close room through a small hole, and receiving the image of it upon a plane perpendicularly. For by this means he hath a triangle, whose sides and angles he can know by measure; and the vertical angle he seeks for, and the substance of the arc of the sun's body.

A. But I think it impossible to distinguish where the part illuminate toucheth the part not illuminate.

B. Another way is this: upon the equinoctial day, with a watch that shows the minutes standing by you, observe when the lower brim of the sun's setting first comes to the horizon, and set the index to some minute of the watch; and observe again the upper brim when it comes to the horizon: then count the minutes, and you have what you look for. Other way I know none.

CHAPTER V.

OF THE MOTIONS OF WATER AND AIR.

CHAP. V. Of the motions of water and air.

A. I HAVE considered, as you bad me, this compounded motion with great admiration. First, it is that which makes the difference between *continuum* and *contiguum*, which till now I never could distinguish. For bodies that are but contiguous, with any little force are parted; but by this compounded motion (because every point of the body makes an equal line in equal time, and every line crosses all the rest) one part cannot be separated from another, without disturbing the motion of all the other parts at once. And is not that the cause, think you, that some bodies when they are pressed or bent, as soon as the force is removed, return again of themselves to their former figure?

B. Yes, sure; saving that it is not of themselves that they return, (for we were agreed that nothing can move itself), but it is the motion of the parts which are not pressed, that delivers those that are. And this restitution the learned now call the spring of a body. The Greeks called it *antitypia*.

A. When I considered this motion in the sun and the earth and planets, I fancied them as so many bodies of the army of the Almighty in an immense field of air, marching swiftly, and commanded (under God) by his glorious officer the sun, or rather forced so to keep their order in every part of every of those bodies, as never to go out from the distance in which he had set them.

B. But the parts of the air and other fluids keep not their places so.

Of the motions of water and air.

A. No: you told me that this motion is not natural in the air, but received from the sun.

B. True: but since we seek the natural causes of sublunary effects, where shall we begin?

A. I would fain know what makes the sea to ebb and flow at certain periods, and what causeth such variety in the tides.

B. Remember that the earth turneth every day upon its own axis from west to east; and all the while it so turneth, every point thereof by its compounded motion makes other circlings, but not on the same centre, which is (you know) a rising in one part of the day, and a falling in the other part. What think you must happen to the sea, which resteth on it, and is a fluid body?

A. I think it must make the sea rise and fall. And the same happeneth also to the air, from the motion of the sun.

B. Remember, also, in what manner the sea is situated in respect of the dry land.

A. Is not there a great sea that reacheth from the straits of Magellan eastward to the Indies, and thence to the same straits again? And is not there a great sea called the Atlantic sea that runneth northward to us? And does not the great south sea run also up into the northern seas? But I think the Indian and the South sea of themselves to be greater than all the rest of the surface of the globe.

B. How lieth the water in those two seas?

A. East and west, and rises and falls a little, as it is forced to do by this compounded motion, which is a kind of succussion of the earth, and fills both the Atlantic and Northern seas.

B. All this would not make a visible difference between high and low water, because this motion being so regular, the unevenness would not be great enough to be seen. For though in a basin the water would be thrown into the air, yet the earth cannot throw the sea into the air.

A. Yes; the basin, if gently moved, will make the water so move, that you shall hardly see it rise.

B. It may be so. But you should never see it rise as it doth, if it were not checked. For at the straits of Magellan, the great South sea is checked by the shore of the continent of Peru and Chili, and forced to rise to a great height, and made to run up into the northern seas on that side by the coast of China; and at the return is checked again and forced through the Atlantic into the British and German seas. And this is done every day. For we have supposed that the earth's motion in the ecliptic caused by the sun is annual; and that its motion in the equinoctial is diurnal. It followeth therefore from this compounded motion of the earth, the sea must ebb and flow twice in the space of twenty-four hours, or thereabout.

A. Has the moon nothing to do in this business?

B. Yes. For she hath also the like motion. And is, though less swift, yet much nearer to the earth. And therefore when the sun and moon are in conjunction or opposition, the earth, as from two agents at once, must needs have a greater succussion. And if it chance at the same time the moon also be in the ecliptic, it will be yet greater, because the moon then worketh on the earth less obliquely.

A. But when the full or new moon happen to be then when the earth is in the equinoctial points, the tides are greater than ordinary. Why is that?

B. Because then the force by which they move the sea, is at that time, to the force by which they move the same at other times, as the equinoctial circle to one of its parallels, which is a lesser circle.

A. It is evident. And it is pleasant to see the concord of so many and various motions, when they proceed from one and the same hypothesis. But what say you to the stupendous tides which happen on the coasts of Lincolnshire on the east, and in the river of Severn on the west?

B. The cause of that, is their proper situation. For the current of the ocean through the Atlantic sea, and the current of the south sea through the northern seas, meeting together, rise the water in the Irish and British seas a great deal higher than ordinary. Therefore the mouth of the Severn being directly opposite to the current from the Atlantic sea, and those sands on the coast of Lincolnshire directly opposite to the current of the German sea, those tides must needs fall furiously into them, by this succussion of the water.

A. Does, when the tide runs up into a river, the water all rise together, and fall together when it goes out?

B. No: one part riseth and another falleth at the same time; because the motion of the earth rising and falling, is that which makes the tide.

A. Have you any experiment that shows it?

B. Yes. You know that in the Thames, it is high water at Greenwich before it is high water at London-bridge. The water therefore falls at Greenwich whilst it riseth all the way to London.

CHAP. V.

Of the motions of water and air.

But except the top of the water went up, and the lower part downward, it were impossible.

A. It is certain. It is strange that this one motion should salve so many appearances, and so easily. But I will produce one experiment of water, not in the sea, but in a glass. If you can show me that the cause of it is this compounded motion, I shall go near to think it the cause of all other effects of nature hitherto disputed of. The experiment is common, and described by the Lord Chancellor Bacon, in the third page of his natural history. Take, saith he, a glass of water, and draw your finger round about the lip of the glass, pressing it somewhat hard; after you have done so a few times, it will make the water frisk up into a fine dew. After I had read this, I tried the same with all diligence myself, and found true not only the frisking of the water to above an inch high, but also the whole superficies to circulate, and withal to make a pleasant sound. The cause of the frisking he attributes to a tumult of the inward parts of the substance of the glass striving to free itself from the pressure.

B. I have tried and found both the sound and motion; and do not doubt but the pressure of the parts of the glass was part of the cause. But the motion of my finger about the glass was always parallel; and when it chanced to be otherwise, both sound and motion ceased.

A. I found the same. And being satisfied, I proceed to other questions. How is the water, being a heavy body, made to ascend in small particles into the air, and be there for a time sustained in form of a cloud, and then fall down again in rain?

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B. I have shown already, that this compounded motion of the sun, in one part of its circumlation, drives the air one way, and in the other part, the contrary way; and that it cannot draw it back again, no more than he that sets a stone a flying can pull it back. The air therefore, which is contiguous to the water, being thus distracted, must either leave a vacuum, or else some part of the water must rise and fill the spaces continually forsaken by the air. But, that there is no vacuum, you have granted. Therefore the water riseth into the air, and maketh the clouds; and seeing they are very small and invisible parts of the water, they are, though naturally heavy, easily carried up and down with the wind, till, meeting with some mountain or other clouds, they be pressed together into greater drops, and fall by their weight. So also it is forced up in moist ground, and with it many small atoms of the earth, which are either twisted with the rising water into plants, or are carried up and down in the air incertainly. But the greatest quantity of water is forced up from the great South and Indian Seas, that lie under the tropic of capricorn. And this climate is that which makes the sun's perigæum to be always on the winter-solstice. And that is the part of the terrestrial globe which Kepler says is kind to the sun; whereas the other part of the globe, which is almost all dry land, has an antipathy to the sun. And so you see where this magnetical virtue of the earth lies. For the globe of the earth having no natural appetite to any place, may be drawn by this motion of the sun a little nearer to it, together with the water which it raiseth.

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A. Can you guess what may be the cause of wind?

B. I think it manifest that the unconstant winds proceed from the uncertain motion of the clouds ascending and descending, or meeting with one another. For the winds after they are generated in any place by the descent of a cloud, they drive other clouds this way and that way before them, the air seeking to free itself from being pent up in a strait. For when a cloud descendeth, it makes no wind sensible directly under itself. But the air between it and the earth is pressed and forced to move violently outward. For it is a certain experiment of mariners, that if the sea go high when they are becalmed, they say they shall have more wind than they would; and take in their sails all but what is necessary for steering. They know, it seems, that the sea is moved by the descent of clouds at some distance off: which presseth the water, and makes it come to them in great waves. For a horizontal wind does but curl the water.

A. From whence come the rivers?

B. From the rain, or from the falling of snow on the higher ground. But when it descendeth under ground, the place where it again ariseth is called the spring.

A. How then can there be a spring upon the top of a hill?

B. There is no spring upon the very top of a hill, unless some natural pipe bring it thither from a higher hill.

A. Julius Scaliger says, there is a river, and in it a lake, upon the top of Mount Cenis in Savoy; and will therefore have the springs to be ingendered

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in the caverns of the earth by condensation of the air.

B. I wonder he should say that. I have passed over that hill twice since the time I read that in Scaliger, and found that river as I passed, and went by the side of it in plain ground almost two miles; where I saw the water from two great hills, one on one side, the other on the other, in a thousand small rillets of melting snow fall down into it. Which has made me never to use any experiment the which I have not myself seen. As for the conversion of air into water by condensation, and of water into air by rarefaction, though it be the doctrine of the Peripatetics, it is a thing incogitable, and the words are insignificant. For by densum is signified only frequency and closeness of parts; and by rarum the contrary. As when we say a town is thick with houses, or a wood with trees, we mean not that one house or tree is thicker than another, but that the spaces between are not so great. But, since there is no vacuum, the spaces between the parts of air are no larger than between the parts of water, or of any thing else.

A. What think you of those things which mariners that have sailed through the Atlantic Sea, called *spouts*, which pour down water enough at once to drown a great ship?

B. It is a thing I have not seen: and therefore can say nothing to it; though I doubt not but when two very large and heavy clouds shall be driven together by two great and contrary winds, the thing is possible.

A. I think your reason good. And now I will propound to you another experiment. I have seen

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an exceeding small tube of glass with both ends open, set upright in a vessel of water, and that the superficies of the water within the tube was higher a good deal than of that in the vessel; but I see no reason for it.

B. Was not part of the glass under water? Must not then the water in the vessel rise? Must not the air that lay upon it rise with it? Whither should this rising air go, since there is no place empty to receive it? It is therefore no wonder if the water, pressed by the substance of the glass which is dipt into it, do rather rise into a very small pipe, than come about a longer way into the open air.

A. It is very probable. I observed also that the top of the inclosed water was a concave superficies; which I never saw in other fluids.

B. The water hath some degree of tenacity, though not so great but that it will yield a little to the motion of the air; as is manifest in the bubbles of water, where the concavity is always towards the air. And this I think the cause why the air and water meeting in the tube make the superficies towards the air concave, which it cannot do to a fluid of greater tenacity.

A. If you put into a basin of water a long rag of cloth, first drenched in water, and let the longer part of it hang out, it is known by experience, that the water will drop out as long as there is any part of the other end under water.

B. The cause of it is, that water, as I told you, hath a degree of tenacity. And therefore being continued in the rag till it be lower without than within, the weight will make it continue dropping, CHAP. V.

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though not only because it is heavy (for if the rag lay higher without than within, and were made heavier by the breadth, it would not descend), but it is because all heavy bodies naturally descend with proportion of swiftness duplicate to that of the time; whereof I shall say more when we talk of gravity.

A. You see how despicable experiments I trouble you with. But I hope you will pardon me.

B. As for mean and common experiments, I think them a great deal better witnesses of nature, than those that are forced by fire, and known but to very few.

CHAPTER VI.

OF THE CAUSES AND EFFECTS OF HEAT AND COLD.

A. It is a fine day, and pleasant walking through the fields, but that the sun is a little too hot.

B. How know you that the sun is hot?

A. I feel it.

B. That is to say, you know that yourself, but not that the sun is hot. But when you find yourself hot, what body do you feel?

A. None.

B. How then can you infer your heat from the sense of feeling? Your walking may have made you hot: is motion therefore hot? No. You are to consider the concomitants of your heat; as, that you are more faint, or more ruddy, or that you sweat, or feel some endeavour of moisture or spirits tending outward; and when you have found the causes of those accidents, you have found the

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causes of heat, which in a living creature, and especially in a man, is many times the motion of the parts within him, such as happen in sickness, anger, and other passions of the mind; which are not in the sun nor in fire.

A. That which I desire now to know, is what motions and of what bodies without me are the efficient causes of my heat.

B. I showed you yesterday, in discoursing of rain, how by this compounded motion of the sun's body, the air was every way at once thrust off west and east; so that where it was contiguous, the small parts of the water were forced to rise, for the avoiding of vacuum. Think then that your hand were in the place of water so exposed to the sun. Must not the sun work upon it as it did upon the water? Though it break not the skin, yet it will give to the inner fluids and looser parts of your hand, an endeavour to get forth, which will extend the skin, and in some climates fetch up the blood, and in time make the skin black. The fire also will do the same to them that often sit with their naked skins too near it. Nay, one may sit so near, without touching it, as it shall blister or break the skin, and fetch up both spirits and blood mixt into a putrid oily matter, sooner than in a furnace oil can be extracted out of a plant.

A. But if the water be above the fire in a kettle, what then will it do? Shall the particles of water go toward the fire, as it did toward the sun?

B. No. For it cannot. But the motion of the parts of the kettle which are caused by the fire, shall dissipate the water into vapour till it be all cast out.

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A. What is that you call fire? Is it a hard or fluid body?

B. It is not any other body but that of the shining coal; which coal, though extinguished with water, is still the same body. So also in a very hot furnace, the hollow spaces between the shining coals, though they burn that you put into them, are no other body than air moved.

A. Is it not flame?

B. No. For flame is nothing but a multitude of sparks, and sparks are but the atoms of the fuel dissipated by the incredible swift motion of the movent, which makes every spark to seem a hundred times greater than it is, as appears by this; that, when a man swings in the air a small stick fired at one end, though the motion cannot be very swift, yet the fire will appear to the eye to be a long, straight, or crooked line. Therefore a great many sparks together flying upward, must needs appear unto the sight as one continued flame. Nor are the sparks stricken out of a flint any thing else but small particles of the stone, which by their swift motion are made to shine. But that fire is not a substance of itself, is evident enough by this, that the sun-beams passing through a globe of water will burn as other fire does. Which beams, if they were indeed fire, would be quenched in the passage.

A. This is so evident, that I wonder so wise men as Aristotle and his followers, for so long a time could hold it for an element, and one of the primary parts of the universe. But the natural heat of a man or other living creature, whence proceedeth it? Is there anything within their bodies that hath this compounded motion?

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B. At the breaking up of a deer I have seen it plainly in his bowels as long as they were warm. And it is called the peristaltic motion, and in the heart of a beast newly taken out of his body; and this motion is called systole and diastole. But they are both of them this compounded motion, whereof the former causeth the food to wind up and down through the guts, and the latter makes the circulation of the blood.

A. What kind of motion is the cause of cold? Methinks it should be contrary to that which causeth heat.

B. So it is in some respect. For seeing the motion that begets heat, tendeth to the separation of the parts of the body whereon it acteth, it stands with reason, that the motion which maketh cold, should be such as sets them closer together. But contrary motions are, to speak properly, when upon two ends of a line two bodies move towards each other, the effect whereof is to make them meet. But each of them, as to this question, is the same.

A. Do you think (as many philosophers have held and now hold), that cold is nothing but a privation of heat?

B. No. Have you never heard the fable of the satyr that dwelling with a husbandman, and seeing him blow his fingers to warm them, and his pottage to cool it, was so scandalized, that he ran from him, saying he would no longer dwell with one that could blow both hot and cold with one breath? Yet the cause is evident enough. For the air which had gotten a calefactive power from his vital parts, was from his mouth and throat gently

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diffused on his fingers, and retained still that power. But to cool his pottage he straightened the passage at his lips, which extinguished the calefactive motion.

A. Do you think wind the general cause of cold? If that were true, in the greatest winds we should have the greatest frosts.

B. I mean not any of those uncertain winds which, I said, were made by the clouds, but such as a body moved in the air makes to and against itself; (for it is all one motion of the air whether it be carried against the body, or the body against it); such a wind as is constant, if no other be stirring, from east to west; and made by the earth turning daily upon its own centre; which is so swift, as, except it be kept off by some hill, to kill a man, as by experience hath been found by those who have passed over great mountains, and specially over the Andes which are opposed to the east. And such is the wind which the earth maketh in the air by her annual motion, which is so swift, as that, by the calculation of astronomers, to go sixty miles in a minute of an hour. And therefore this must be the motion which makes it so cold about the poles of the ecliptic.

A. Does not the earth make the wind as great in one part of the ecliptic as in another?

B. Yes. But when the sun is in Cancer, it tempers the cold, and still less and less, but least of all in the winter-solstice, where his beams are most oblique to the superficies of the earth.

A. I thought the greatest cold had been about the poles of the equator.

B. And so did I once. But the reason commonly

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given for it is so improbable, that I do not think so now. For the cause they render of it is only, that the motion of the earth is swiftest in the equinoctial, and slowest about the poles; and consequently, since motion is the cause of heat, and cold is but, as it was thought, a want of the same, they inferred that the greatest cold must be about the poles of the equinoctial. Wherein they miscounted. For not every motion causeth heat, but this agitation only, which we call compounded motion; though some have alleged experience for that opinion; as that a bullet out of a gun will with its own swiftness melt. Which I never shall believe.

A. It is a common thing with many philosophers to maintain their fancies with any rash report, and sometimes with a lie. But how is it possible that so soft a substance as water should be turned into so hard a substance as ice?

B. When the air shaves the globe of the earth with such swiftness, as that of sixty miles in a minute of an hour, it cannot, where it meets with still water, but beat it up into small and undistinguishable bubbles, and involve itself in them as in so many bladders or skins of water. And ice is nothing else but the smallest imaginable parts of air and water mixed; which is made hard by this compounded motion, that keeps the parts so close together, as not to be separated in one place without disordering the motion of them all. For when body will not easily yield to the impression of an ternal movent in one place without yielding in we call it hard; and when it does, we say it is

. Why is not ice as well made in a moved as in

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a still water? Are there not great seas of ice in the northern parts of the earth?

B. Yes, and perhaps also in the southern parts. But I cannot imagine how ice can be made in such agitation as is always in the open sea, made by the tides and by the winds. But how it may be made at the shore, it is not hard to imagine. For in a river or current, though swift, the water that adhereth to the banks is quiet, and easily by the motion of the air driven into small insensible bubbles; and so may the water that adhereth to those bubbles, and so forwards till it come into a stream that breaks it, and then it is no wonder though the fragments be driven into the open sea, and freeze together into greater lumps. But when in the open sea, or at the shore, the tide or a great wave shall arise, this young and tender ice will presently be washed away. And therefore I think it evident, that as in the Thames the ice is first made at the banks where the tide is weak or none, and, broken by the stream, comes down to London, and part goes to the sea floating till it dissolve, and part, being too great to pass the bridge, stoppeth there and sustains that which follows, till the river be quite frozen over; so also the ice in the northern seas begins first at the banks of the continent and islands which are situated in that climate, and then broken off, are carried up and down, and one against another, till they become great bodies.

A. But what if there be islands, and narrow inlets of the sea, or rivers also about the pole of the equinoctial?

B. If there be, it is very likely the sea may also there be covered all over with ice. But for the

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truth of this, we must stay for some farther discovery.

A. When the ice is once made and hard, what dissolves it?

B. The principal cause of it, is the weight of the water itself; but not without some abatement in the stream of the air that hardeneth it; as when the sunbeams are less oblique to the earth, or some contrary wind resisteth the stream of the air. For when the impediment is removed, then the nature of the water only worketh, and, being a heavy body, downward.

A. I forgot to ask you, why two pieces of wood rubbed swiftly one against another, will at length set on fire.

B. Not only at length, but quickly, if the wood be dry. And the cause is evident, viz. the compounded motion which dissipates the external small parts of the wood. And then the inner parts must of necessity, to preserve the plentitude of the universe, come after; first the most fluid, and then those also of greater consistence, which are first erected, and the motion continued, made to fly swiftly out; whereby the air driven to the eye of the beholder, maketh that fancy which is called light.

A. Yes; I remember you told me before, that upon any strong pressure of the eye, the resistance from within would appear a light. But to return to the enquiry of heat and cold, there be two things that beyond all other put me into admiration. One is the swiftness of kindling in gunpowder. The other is the freezing of water in a vessel, though not far from the fire, set about with other water with ice and snow in it. When paper or flax is flaming, the flame creeps gently on; and if a house full of paper were to be burnt with putting a candle to it, it will be long in burning; whereas a spark of fire would set on flame a mountain of gunpowder in almost an instant.

B. Know you not gunpowder is made of the powder of charcoal, brimstone, and saltpetre? Whereof the first will kindle with a spark, the second flame as soon as touched with fire; and the third blows it, as being composed of many orbs of salt filled with air, and as it dissolveth in the flame, furiously blowing increaseth it. And as for making ice by the fireside; it is manifest that whilst the snow is dissolving in the external vessel, the air must in the like manner break forth, and shave the superficies of the inner vessel, and work through the water till it be frozen.

A. I could easily assent to this, if I could conceive how the air that shaves, as you say, the outside of the vessel, could work through it. I conceive well enough a pail of water with ice or snow dissolving in it, and how it causeth wind. But how that wind should communicate itself through the vessel of wood or metal, so as to make it shave the superficies of the water which is within it, I do not so well understand.

B. I do not say the inner superficies of the vessel shaves the water within it. But it is manifest that the wind made in the pail of water by the melting snow or ice presseth the sides of the vessel that standeth in it; and that the pressure worketh clean through, how hard soever the vessel be; and that again worketh on the water within, by resti-

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tution of its parts, and so hardeneth the water by degrees.

A. I understand you now. The ice in the pail by its dissolution transfers its hardness to the water within.

B. You are merry. But supposing, as I do, that the ice in the pail is more than the water in the vessel, you will find no absurdity in the argument. Besides, the experiment, you know, is common.

A. I confess it is probable. The Greeks have the word φρικη (whence the Latins have their word *frigus*) to signify the curling of the water by the wind; and use the same also for horror, which is the passion of one that cometh suddenly into a cold air, or is put into a sudden affright, whereby he shrinks, and his hair stands upright. Which manifestly shows that the motion which causeth cold, is that which pressing the superficies of a body, sets the parts of it closer together. But to proceed in my queries. Monsieur Des Cartes, whom you know, hath written somewhere, that the noise we hear in thunder, proceeds from breaking of the ice in the clouds; what think you of it? Can a cloud be turned into ice?

B. Why not? A cloud is but water in the air?

A. But how? For he has not told us that.

B. You know that it is only in summer, and in hot weather, that it thunders; or if in winter, it is taken for a prodigy. You know also, that of clouds, some are higher, some lower, and many in number, as you cannot but have oftentimes observed, with spaces between them. Therefore, as in all currents of water, the water is there swiftest where it is straitened with islands, so must the current of

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air made by the annual motion be swiftest there, where it is checked with many clouds, through which it must, as it were, be strained, and leave behind it many small particles of earth always in it, and in hot weather more than ordinary.

A. This I understand, and that it may cause ice. But when the ice is made, how is it broken? And why falls it not down in shivers?

B. The particles are enclosed in small caverns of the ice; and their natural motion being the same which we have ascribed to the globe of the earth, requires a sufficient space to move in. But when it is imprisoned in a less room than that, then a great part of the ice breaks: and this is the thunder-clap. The murmur following is from the settling of the air. The lightning is the fancy made by the recoiling of the air against the eye. The fall is in rain, not in shivers; because the prisons which they break are extreme narrow, and the shivers being small, are dissolved by the heat. But in less heat they would fall in drops of hail, that is to say, half frozen by the shaving of the air as they fall, and be in a very little time, much less than snow or ice, dissolved.

A. Will not that lightning burn?

B. No. But it hath often killed men with cold. But this extraordinary swiftness of lightning consisteth not in the expansion of the air, but in a straight and direct stream from where it breaks forth; which is in many places successively, according to the motion of the cloud.

A. Experience tells us that. I have now done with my problems concerning the great bodies of the world, the stars, and element of air in which

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they are moved, and am therein satisfied, and the rather, because you have answered me by the supposition of one only motion, and commonly known, and the same with that of Copernicus, whose opinion is received by all the learned; and because you have not used any of these empty terms, sympathy, antipathy, antiperistasis, etc., for a natural cause, as the old philosophers have done to save their credit. For though they were many of them wise men, as Plato, Aristotle, Seneca, and others, and have written excellently of morals and politics, yet there is very little natural philosophy to be gathered out of their writings.

B. Their ethics and politics are pleasant reading, but I find not any argument in their discourses of justice or virtue drawn from the supreme authority, on whose laws all justice, virtue, and good politics depend.

A. Concerning this cover, or, as some have called it, the scurf or scab of the terrestrial star, I will begin with you tomorrow. For it is a large subject, containing animals, vegetables, metals, stones, and many other kinds of bodies, the knowledge whereof is desired by most men, and of the greatest and most general profit.

B. And this is it, in which I shall give you the least satisfaction; so great is the variety of motion, and so concealed from human senses.

CHAPTER VII.

OF HARD AND SOFT, AND OF THE ATOMS THAT FLY IN THE AIR.

CHAP. VII.

Of hard and soft, and of the atoms that fly in the air.

A. CONCERNING this cover of the earth, made up of an infinite number of parts of different natures, I had much ado to find any tolerable method of enquiry. But I resolved at last to begin with the questions concerning hard and soft, and what kind of motion it is that makes them so. I know that in any pulsion of air, the parts of it go innumerable and inexplicable ways; but I ask only if every point of it be moved?

B. No. If you mean a mathematical point, you know it is impossible. For nothing is movable but body. But I suppose it divisible, as all other bodies, into parts divisible. For no substance can be divided into nothings.

A. Why may not that substance within our bodies, which are called animal spirits, be another kind of body, and more subtile than the common air?

B. I know not why, no more than you or any man else knows why it is not very air, though purer perhaps than the common air, as being strained through the blood into the brain and nerves. But howsoever that be, there is no doubt, but the least parts of the common air, respectively to the whole, will easilier pierce, with equal motion, the body that resisteth them, than the least parts of water. For it is by motion only that any mutation is made in any thing; and all things standing as they did, will appear as they did. And that which changeth soft into hard, must be such as

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makes the parts not easily to be moved without being moved all together; which cannot be done but by some motion compounded. And we call hard, that whereof no part can be put out of order without disordering all the rest; which is not easily done.

A. How water and air beaten into extreme small bubbles is hardened into ice, you have told me already, and I understand it. But how a soft homogeneous body, as air or water, should be so hardened, I cannot imagine.

B. There is no hard body that hath not also some degree of gravity; and consequently, being loose, there must be some efficient cause, that is, some motion, when it is severed from the earth, to bring the same to it again. And seeing this compounded motion gives to the air and water an endeavour from the earth, the motion which must hinder it, must be in a way contrary to the compounded motion of the earth. For whatsoever, having been asunder, comes together again, must come contrary ways, as those that follow one another go the same way, though both move upon the same line.

A. What experiment have you seen to this purpose?

B. I have seen a drop of glass like that of the second figure, newly taken out of the furnace, and hanging at the end of an iron rod, and yet fluid, and let fall into the water and hardened. The club-end of it A A coming first to the water, the tail B C following it. It is proved before, that the motion that makes it is a compounded motion, and gives an endeavour outward to every part of it; and that the motion which maketh cold, is such as

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shaving the body in every point of contact, and turning it, gives them all an endeavour inward. Such is this motion made by the sinking of the hot and fluid glass into the water. It is therefore manifest that the motion which hardeneth a soft body, must in every point of contact be in the contrary way to that which makes a hard body soft. And farther, that slender tail B C shall be made much more hard than common glass. For towards the upper end, in C, you cannot easily break it, as small as it is. And when you have broken it, the whole body will fall into dust, as it must do, seeing the bending is so difficult. For all the parts are bent with such force, that upon the breaking at D, by their sudden restitution to their liberty, they will break together. And the cause why the tail B C, being so slender, becomes so hard, is, that all the endeavour in the great part A B, is propagated to the small part B C, in the same manner as the force of the sun-beams is derived almost to a point by a burning-glass. But the cause why, when it is broken in D, it breaks also in so many other places, is, that the endeavour in all the other parts, which is called the spring, unbends it; from whence a motion is caused the contrary way, and that motion continued bends it more the other way and breaks it, as a bow over-bent is broken into shivers by a sudden breaking of the string.

A. I conceive now how a body which having been hard and softened again, may be rehardened; but how a fluid and mere homogeneous body, as air or water, may be so, I see not yet. For the hardening of water is making a hard body of two fluids, whereof one, which is the water, hath some tena-

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city; and so a man may make a bladder hard with blowing into it.

B. As for mere air, which hath no natural motion of itself, but is moved only by other bodies of a greater consistence, I think it impossible to be hardened. For the parts of it so easily change places, that they can never be fixed by any motion. No more I think can water, which though somewhat less fluid, is with an insensible force very easily broken.

A. It is the opinion of many learned men, that ice, in long time, will be turned into crystal; and they allege experience for it. For they say that crystal is found hanging on high rocks in the Alps, like icicles on the eaves of a house; and why may not that have formerly been ice, and in many years have lost the power of being reduced?

B. If that were so, it would still be ice, though also crystal: which cannot be, because crystal is heavier than water, and therefore much heavier than ice.

A. Is there then no transubstantiation of bodies but by mixture?

B. Mixture is no transubstantiation.

A. Have you never seen a stone that seemed to have been formerly wood, and some like shells, and some like serpents, and others like other things?

B. Yes. I have seen such things, and particularly I saw at Rome, in a stone-cutter's workhouse, a billet of wood, as I thought it, partly covered with bark, and partly with the grain bare, as long as a man's arm, and as thick as the calf of a man's leg; which handling I found extreme heavy, and saw a small part of it which was polished, and had a very

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fine gloss, and thought it a substance between stone and metal, but nearest to stone. I have seen also a kind of slate painted naturally with forest-work. And I have seen in the hands of a chemist of my acquaintance at Paris, a broken glass, part of a retort, in which had been the rosin of turpentine, wherein though there were left no rosin, yet there appeared in the piece of glass many trees; and plants in the ground about them, such as grow in woods; and better designed than they could be done by any painter; and continued so for a long time. These be great wonders of nature, but I will not undertake to show their causes. But yet this is most certain, that nothing can make a hard body of a soft, but by some motion of its parts. For the parts of the hardest body in the world can be no closer together than to touch; and so close are the parts of air and water, and consequently they should be equally hard, if their smallest parts had not different natural motions. Therefore if you ask me the causes of these effects, I answer, they are different motions. But if you expect from me how and by what motions, I shall fail you. For there is no kind of substance in the world now, that was not at the first creation, when the Creator gave to all things what natural and special motion he thought good. And as he made some bodies wondrous great, so he made others wondrous little. For all his works are wondrous. Man can but guess, nor guess farther, than he hath knowledge of the variety of motion. I am therefore of opinion, that whatsoever perfectly homogeneous is hard, consisteth of the smallest parts, or, as some call them, atoms, that were made hard in

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the beginning, and consequently by an eternal cause; and that the hardness of the whole body is caused only by the contact of the parts by pressure.

A. What motion is it that maketh a hard body to melt?

B. The same compounded motion that heats, namely, that of fire, if it be strong enough. For all motion compounded is an endeavour to dissipate, as I have said before, the parts of the body to be moved by it. If therefore hardness consist only in the pressing contact of the least parts, this motion will make the same parts slide off from one another, and the whole to take such a figure as the weight of the parts shall dispose them to, as in lead, iron, gold, and other things melted with heat. But if the small parts have such figures as they cannot exactly touch, but must leave spaces between them filled with air or other fluids, then this motion of the fire, will dissipate those parts some one way, some another, the hard part still hard; as in the burning of wood or stone into ashes or lime. For this motion is that which maketh fermentation, scattering dissimilar parts, and congregating similar.

A. Why do some hard bodies resist breaking more one way than another?

B. The bodies that do so, are for the most part wood, and receive that quality from their generation. For the heat of the sun in the spring-time draweth up the moisture at the root, and together with it the small parts of the earth, and twisteth it into a small twig, by its motion upwards, to some length, but to very little other dimensions, and so leaves it to dry till the spring following; and then

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does the same to that, and to every small part round about it; so that upward the strength is doubled, and the next year trebled, &c. And these are called the grain of the wood, and but touch one another, like sticks with little or no binding, and therefore can hardly be broken across the grain, but easily all-along it. Also some other hard bodies have this quality of being more fragile one way than another, as we see in quarrels of a glass window, that are aptest many times to break in some crooked line. The cause of this may be, that when the glass, hot from the furnace, is poured out upon a plain, any small stones in or under it will break the stream of it into divers lines, and not only weaken it, but also cause it falsely to represent the object you look on through it.

A. What is the cause why a bow of wood or steel, or other very hard body, being bent, but not broken, will recover its former degree of straightness?

B. I have told you already, how the smallest parts of a hard body have every one, by the generation of hardness, a circular, or other compounded motion; such motion is that of the smallest parts of the bow. Which circles in the bending you press into narrower figures, as a circle into an ellipsis, and an ellipsis into a narrower but longer ellipsis with violence; which turns their natural motion against the outward parts of the bow so bent, and is an endeavour to stretch the bow into its former posture. Therefore if the impediment be removed, the bow must needs recover its former figure.

A. It is manifest; and the cause can be no other but that, except the bow have sense.

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B. And though the bow had sense, and appetite to boot, the cause will be still the same.

A. Do you think air and water to be pure and homogeneous bodies?

B. Yes, and many bodies both hard and heavy to be so too, and many liquors also besides water.

A. Why then do men say they find one air healthy, another infectious?

B. Not because the nature of the air varies, but because there are in the air, drawn, or rather, beaten up by the sun, many little bodies, whereof some have such motion as is healthful, others such as is hurtful to the life of man. For the sun, as you see in the generation of plants, can fetch up earth as well as water: and from the driest ground any kind of body that lieth loose, so it be small enough, rather than admit any emptiness. By some of these small bodies it is that we live; which being taken in with our breath, pass into our blood, and cause it, by their compounded motion, to circulate through the veins and arteries; which the blood of itself, being a heavy body, without it cannot do. What kind of substance these atoms are, I cannot tell. Some suppose them to be nitre. As for those infectious creatures in the air, whereof so many die in the plague, I have heard that Monsieur Des Cartes, a very ingenious man, was of opinion, that they were little flies. But what grounds he had for it, I know not, though there be many experiments that invite me to believe it. For first, we know that the air is never universally infected over a whole country, but only in or near to some populous town. And therefore the cause must also be partly ascribed to the multitude

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thronged together, and constrained to carry their excrements into the fields round about and near to their habitation, which in time fermenting breed worms, which commonly in a month or little more, naturally become flies; and though engendered at one town, may fly to another. Secondly, in the beginning of a plague, those that dwell in the suburbs, that is to say, nearest to this corruption, are the poorest of the people, that are nourished for the most part with the roots and herbs which grow in that corrupted dirt; so that the same filth makes both the blood of poor people, and the substance of the fly. And it is said by Aristotle, that everything is nourished by the matter whereof it is generated. Thirdly, when a town is infected, the gentlemen, and those that live on wholsomest food, scarce one of five hundred die of the plague. It seems therefore, whatsoever creatures they be that invade us from the air, they can discern their proper nourishment, and do not enter into the mouth and nostrils with the breath of every man alike, as they would do if they were inanimate. Fourthly, a man may carry the infection with him a great way into the country in his clothes, and infect a village. Shall another man there draw the infection from the clothes only by his breath? Or from the hangings of a chamber wherein a man hath died? It is impossible. Therefore whatsoever killing thing is in the clothes or hangings, it must rise and go into his mouth or nostrils before it can do him hurt. It must therefore be a fly, whereof great numbers get into the blood, and there feeding and breeding worms, obstruct the circulation of blood, and kill the man.

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A. I would we knew the palate of those little animals; we might perhaps find some medicine to fright them from mingling with our breath. But what is that which kills men that lie asleep too near a charcoal-fire? Is it another kind of fly? Or is charcoal venomous?

B. It is neither fly nor venom, but the effect of a flameless glowing fire, which dissipates those atoms that maintain the circulation of the blood; so that for want of it, by degrees they faint, and being asleep cannot remove, but in short time, there sleeping die; as is evident by this, that being brought into the open air, without other help, they recover.

A. It is very likely. The next thing I would be informed of, is the nature of gravity. But for that, if you please, we will take another day.

CHAPTER VIII.

OF GRAVITY AND GRAVITATION.

B. WHAT books are those?

A. Two books written by two learned men concerning gravity. I brought them with me, because they furnish me with some material questions about that doctrine; though of the nature of gravity, I find no more in either of them than this, that gravity is an intrinsical quality, by which a body so qualified descendeth perpendicularly towards the superficies of the earth.

B. Did neither of them consider that descending is local motion; that they might have called it an intrinsical motion rather than an intrinsical quality?

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A. Yes. But not how motion should be intrinsical to the special individual body moved. For how should they, when you are the first that ever sought the differences of qualities in local motion, except your authority in philosophy were greater with them than it is? For it is hard for a man to conceive, except he see it, how there should be motion within a body, otherwise than as it is in living creatures.

B. But it may be they never sought, or despaired of finding what natural motion could make any inanimate thing tend one way rather than another.

A. So it seems. But the first of them inquires no farther than, why so much water, being a heavy body, as lies perpendicularly on a fish's back in the bottom of the sea, should not kill it. The other, whereof the author is Dr. Wallis, treateth universally of gravity.

B. Well; but what are the questions which from these books you intend to ask me?

A. The author of the first book tells me, that water and other fluids are bodies continued, and act, as to gravity, as a piece of ice would do of the same figure and quantity. Is that true?

B. That the universe, supposing there is no place empty, is one entire body, and also, as he saith it is, a continual body, is very true. And yet the parts thereof may be contiguous, without any other cohesion but touch. And it is also true, that a vessel of water will descend in a medium less heavy, but fluid, as ice would do.

A. But he means that water in a tub would have the same effect upon a fish in the bottom of the tub, as so much ice would have.

B. That also would be true, if the water were frozen to the sides of it. Otherwise the ice, if there be enough, will crush the fish to death. But how applies he this, to prove that the water cannot hurt a fish in the sea by its weight?

A. It plainly appears that water does not gravitate on any part of itself beneath it.

B. It appears by experience, but not by this argument, though instead of water the tub were filled with quicksilver.

A. I thought so. But how it comes to pass that the fish remains uncrushed, I cannot tell.

B. The endeavour of the quicksilver downward is stopped by the resistance of the hard bottom. But all resistance is a contrary endeavour; that is, an endeavour upwards, which gives the like endeavour to the quicksilver, which is also heavy, and thereby the endeavour of the quicksilver is diverted to the sides round about, where stopped again by the resistance of the sides, it receives an endeavour upwards, which carries the fish to the top, lying all the way upon a soft bed of quicksilver. This is the true manner how the fish is saved harmless. But your author, I believe, either wanted age, or had too much business, to study the doctrine of motion; and never considered that resistance is not an impediment only, or privation, but a contrary motion; and that when a man claps two pieces of wax together, their contrary endeavour will turn both the pieces into one cake of wax.

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A. I know not the author; but it seems he has deeplier considered this question than other men; for in the introduction to his book he saith, "that men have pre-engaged themselves to maintain certain principles of their own invention, and are therefore unwilling to receive anything that may render their labour fruitless;" and, "that they have not strictly enough considered the several interventions that abate, impede, advance, or direct the gravitation of bodies."

B. This is true enough; and he himself is one of those men, in that he considered not, that resistance is one of those interventions which abate, impede, and direct gravitation. But what are his suppositions for the questions he handles?

A. His first is, that as in a pyramid of brick, wherein the bricks are so joined that the uppermost lies everywhere over the joint or cement of the two next below it, you may break down a part and leave a cavity, and yet the bricks above will stand firm and sustain one another by their cross posture: so also it is in wheat, hailshot, sand, or water; and so they arch themselves, and thereby the fish is every way secured by an arch of water over it.

B. That the cause why fishes are not crushed nor hurt in the bottom of the sea by the weight of the water, is the water's arching itself, is very manifest. For if the uppermost orb of the water should descend by its gravity, it would tend toward the centre of the earth, and place itself all the way in a less and lesser orb, which is impossible. For the places of the same body are always equal. But that wheat, sand, hailshot, or loose stones should make a firm arch, is not credible.

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A. The author therefore, it seems, quits it, and taketh a second hypothesis for the true cause, though the former, he saith, be not useless, but contributes its part to it.

B. I see, though he depart from his hypothesis, he looks back upon it with some kindness. What is his second hypothesis?

A. It is, that air and water have an endeavour to motion upward, downward, directly, obliquely, and every way. For air, he saith, will come down his chimney, and in at his door, and up his stairs.

B. Yes, and mine too; and so would water, if I dwelt under water, rather than admit of vacuum. But what of that?

A. Why then it would follow, that those several tendencies or endeavours would so abate, impede, and correct one another, as none of them should gravitate. Which being granted, the fish can take no harm; wherein I find one difficulty, which is this: the water having an endeavour to motion every way at once, methinks it should go no way, but lie at rest; which, he saith, was the opinion of Stevinus, and rejecteth it, saying, it would crush the fish into pieces.

B. I think the water in this case would neither rest nor crush. For the endeavour being, as he saith, intrinsical, and every way, must needs drive the water perpetually outward; that is to say, as to this question, upwards; and seeing the same endeavour in one individual body cannot be more ways at once than one, it will carry it on perpetually without limit, beyond the fixed stars; and so we shall never more have rain.

A. As ridiculous as it is, it necessarily follows.

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B. What are Dr. Wallis's suppositions?

A. He goes upon experiments. And, first, he allegeth this, that water left to itself without disturbance, does naturally settle itself into a horizontal plane.

B. He does not then, as your author and all other men, take gravity for that quality whereby a body tendeth to the centre of the earth.

A. Yes, he defines gravity to be a natural propension towards the centre of the earth.

B. Then he contradicteth himself. For if all heavy bodies tend naturally to one centre, they shall never settle in a plane, but in a spherical superficies. But against this, that such an horizontal plane is found in water by experience, I say it is impossible. For the experiment cannot be made in a basin, but in half a mile at sea experience visibly shows the contrary. According to this, he should think also that a pair of scales should hang parallel.

A. He thinks that too.

B. Let us then leave this experiment. What says he farther concerning gravity?

A. He takes for granted, not as an experiment but an axiom, that nature worketh not by election, but *ad ultimum virium*, with all the power it can.

B. I think he means, (for it is a very obscure passage), that every inanimate body by nature worketh all it can without election; which may be true. But it is certain that men, and beasts, work often by election, and often without election; as when he goes by election, and falls without it. In this sense I grant him, that nature does all it can. But what infers he from it?

CHAP. IV.

Problems of heat and light.

A. I think it is, because the in
visible object, which was made at
of the motion, did last till it was
it will follow that it must be vis
the impressions in all points of
equal.

B. The cause can be no other
spark of fire flying up seems a line
and again by that swift circular m
have supposed for the cause of l
broader than it is. And conseque
every thing must needs seem mu
it is.

A. What are those sparks that
fire?

B. They are small pieces of the
or other fuel loosened and carried
air that cometh up with them. A
guished before their parts be quite
others, are so much soot, and bla
fired again.

A. A spark of fire may be strick
stone. It is not therefore heat t
shining.

B. No it is the motion that mak
and shining; and the stroke mak
For every of those sparks, is a littl
stone, which swiftly moved, impri
motion into the matter prepared,
ceive it.

A. How comes the light of the
most any combustible matter by ref
a convex glass, and by reflection fr

B. The air moved by the sun

Of gravity and gravitation.

A. That naturally every body has every way, if the ways oppose not one another, an endeavour to motion; and consequently, that if a vessel have two holes, one at the side, another at the bottom, the water will run out at both.

B. Does he think the body of water that runs out at the side, and that which runs out at the bottom, is but one and the same body of water?

A. No, sure; he cannot think but that they are two several parts of the whole water in the vessel.

B. What wonder is it then, if two parts of water run two ways at once, or a thousand parts a thousand ways? Does it follow thence that one body can go more than one way at once? Why is he still meddling with things of such difficulty? He will find at last that he has not a genius either for natural philosophy or for geometry. What other suppositions has he?

A. My first author had affirmed, that a lighter body does not gravitate on a heavier; against this Dr. Wallis thus argueth: Let there be a siphon, A B C D, filled with quicksilver to the level A D; if then you pour oil upon A as high as to E, he asketh if the oil in A E, as being heavy, shall not press down the quicksilver a little at A, and make it rise a little at D, suppose to F; and answers himself, that certainly it will; so that it is neither an experiment nor an hypothesis, but only his opinion.

Fig. 8.

B. Whatsoever it be, it is not true; though the r may be pardoned, because the contrary was proved.

Can you prove the contrary?

Yes; for the endeavour of the quicksilver

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both from A and D downward, is stronger than that of the oil downward. If, therefore, the endeavour of the quicksilver were not resisted by the bottom B C, it would fall so, by reason of the acceleration of heavy bodies in their descending, as to leave the oil, so that it should not only not press, but also not touch the quicksilver. It is true, in a pair of scales equally charged with quicksilver, that the addition of a little oil to either scale will make it preponderate. And that was it deceived him.

A. It is evident. The last experiment he cites is the weighing of air in a pair of scales, where it is found manifestly that it has some little weight. For if you weigh a bladder, and put the weight into one scale, and then blow the bladder full of air, and put it into the other scale, the full bladder will outweigh the empty. Must not then the air gravitate?

B. It does not follow. I have seen the experiment just as you describe it, but it can never be thence demonstrated that air has any weight. For as much air as is pressed downward by the weight of the blown bladder, so much will rise from below, and lay itself spherically at the altitude of the centre of gravity of the bladder so blown. So that all the air within the bladder above that centre is carried thither imprisoned, and by violence: and the force that carries it up is equal to that which presseth it down. There must, therefore, be allowed some little counterpoise in the other scale to balance it. Therefore, the experiment proves nothing to his purpose. And whereas they say there be small heavy bodies in the air,

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which make it gravitate, do they think the force which brought them thither cannot hold them there?

A. I leave this question of the fish as clearly resolved, because the water tending every way to one point, which is the centre of the earth, must of necessity arch itself. And now tell me your own opinion concerning the cause of gravity, and why all bodies descend or ascend not all alike. For there can be no more matter in one place than another if the places be equal.

B. I have already showed you in general, that the difference of motion in the parts of several bodies makes the difference of their natures. And all the difference of motions consisteth either in swiftness, or in the way, or in the duration. But to tell you in special why gold is heaviest, and then quicksilver, and then, perhaps, lead, is more than I hope to know, or mean to enquire; for I doubt not but that the species of heavy, hard, opaque, and diaphanous, were all made so at their creation, and at the same time separated from different species. So that I cannot guess at any particular motions that should constitute their natures, farther than I am guided by the experiments made by fire or mixture.

A. You hope not then to make gold by art?

B. No, unless I could make one and the same thing heavier than it was. God hath from the beginning made all the kinds of hard, and heavy, and diaphanous bodies that are, and of such figure and magnitude as he thought fit; but how small soever, they may by accretion become greater in the mine, or perhaps by generation, though we

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know not how. But that gold, by the art of man, should be made of not gold, I cannot understand; nor can they that pretend to show how. For the heaviest of all bodies, by what mixture soever of other bodies, will be made lighter, and not to be received for gold.

A. Why, when the cause of gravity consisteth in motion, should you despair of finding it?

B. It is certain that when any two bodies meet, as the earth and any heavy body will, the motion that brings them to or towards one another, must be upon two contrary ways; and so also it is when two bodies press each other in order to make them hard; so that one contrariety of motion might cause both hard and heavy, but it doth not, for the hardest bodies are not always the heaviest; therefore I find no access that way to compare the causes of different endeavours of heavy bodies to descend.

A. But show me at least how any heavy body that is once above in the air, can descend to the earth, when there is no visible movent to thrust or pull it down.

B. It is already granted, that the earth hath this compounded motion supposed by Copernicus, and that thereby it casteth the contiguous air from itself every way round about. Which air so cast off, must continually, by its nature, range itself in a spherical orb. Suppose a stone, for instance, were taken up from the ground, and held up in the air by a man's hand, what shall come into the place it filled when it lay upon the earth?

A. So much air as is equal to the stone in magnitude, must descend and place itself in an

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orb upon the earth. But then I see that to avoid vacuum, another orb of air of the same magnitude must descend, and place itself in that, and so perpetually to the man's hand; and then so much air as would fill the place must descend in the same manner, and bring the stone down with it. For the stone having no endeavour upward, the least motion of the air, the hand being removed, will thrust it downward.

B. It is just so. And farther, the motion of the stone downward shall continually be accelerated according to the odd numbers from unity; as you know hath been demonstrated by Galileo. But we are nothing the nearer, by this, to the knowledge of why one body should have a greater endeavour downward than another. You see the cause of gravity is this compounded motion with exclusion of vacuum.

A. It may be it is the figure that makes the difference. For though figure be not motion, yet it may facilitate motion, as you see commonly the breadth of a heavy body retardeth the sinking of it. And the cause of it is, that it makes the air have farther to go laterally, before it can rise from under it. For suppose a body of quicksilver falling in the air from a certain height, must it not, going as it does toward the centre of the earth, as it draws nearer and nearer to the earth, become more and re slender, in the form of a solid sector? And have far to go, divide itself into drops? This e of a solid sector is like a needle with the downward, and therefore I should think that ating the motion of it does the same that be done by increasing the endeavour.

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B. Do not you see that this way of facilitating is the same in water, and in all other fluid heavy bodies? Besides, your argument ought to be applicable to the weighing of bodies in a pair of scales, which it is not, for there they have no such figure; it should also hold in the comparison of gravity in hard and fluid bodies.

A. I had not sufficiently considered it. But supposing now, as you do, that both heavy and hard bodies, in their smallest parts, were made so in the creation; yet, because quicksilver is harder than water, a drop of water shall in descending be pressed into a more slender sector than a drop of quicksilver, and consequently the earth shall more easily cast off any quantity of water than the same quantity of quicksilver.

B. This one would think were true; as also that of simple fluid bodies, those whose smallest parts, naturally, without the force of fire, do strongliest cohere, are generally the heaviest. But why then should quicksilver be heavier than stone or steel? Fluidity and hardness are but degrees between greater fluidity and greater hardness. Therefore to the knowledge of what it is that causeth the difference, in different bodies, of their endeavour downward, there are required, if it can be known at all, a great many more experiments than have been yet made. It is not difficult to find why water is heavier than ice, or other body mixed of air and water. But to believe that all bodies are heavier or lighter according to the quantity of air within them, is very hard.

A. I see by this, that the Creator of the world, as by his power he ordered it, so by his wisdom he

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provided it should be never disordered. Therefore leaving this question, I desire to know whether if a heavy body were as high as a fixed star, it would return to the earth.

B. It is hard to try. But if there be this compounded motion in the great bodies so high, such as is in the earth, it is very likely that some heavy bodies will be carried to them. But we shall never know it till we be at the like height.

A. What think you is the reason why a drop of water, though heavy, will stand upon a horizontal plane of dry or unctuous wood, and not spread itself upon it? For let A B, in the sixth figure, be the dry plane, D the drop of water, and D C perpendicular to A B. The drop D, though higher, will not descend and spread itself upon it.

Fig. 6.

B. The reason I think is manifest. For those bodies which are made by beating of water and air together, show plainly that the parts of water have a great degree of cohesion. For the skin of the bubble is water, and yet it can keep the air, though moved, from getting out. Therefore the whole drop of water at D, hath a good deal of cohesion of parts. And seeing A B is an horizontal plane, the way from the contact in D either to A or B is upwards, and consequently there is no endeavour in D either of those ways, but what proceeds from so much weight of water as is able to break that cohesion, which so small a drop is too weak to do. But the cohesion being once broken, as with your finger, the water will follow.

A. Seeing the descent of a heavy body increaseth according to the odd numbers 1, 3, 5, 7, &c. and the aggregates of those numbers, viz. of 1 and 3;

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and 1 and 3 and 5; and of 1 and 3 and 5 and 7, &c. are square numbers, namely 4, 9, 16; the whole swiftness of the descent will be, I think, to the aggregate of so many swiftnesses equal to the first endeavour, as square numbers are to their sides, 1, 2, 3, 4. Is it so?

B. Yes, you know it hath been demonstrated by Galileo.

A. Then if, for instance, you put into a pair of scales equal quantities of quicksilver and water, seeing they are both accelerated in the same proportion, why should not the weight of quicksilver to the weight of water be in duplicate proportions to their first endeavours?

B. Because they are in a pair of scales. For there the motion of neither of them is accelerated. And therefore it will be, as the first endeavour of the quicksilver to the first endeavour of the water, so the whole weight to the whole weight. By which you may see, that the cause which takes away the gravitation of liquid bodies from fish or other lighter bodies within them, can never be derived from the weight.

A. I have one question more to ask concerning gravity. If gravity be, as some define it, an intrinsical quality, whereby a body descendeth towards the centre of the earth, how is it possible that a piece of iron that hath this intrinsical quality should rise from the earth, to go to a loadstone? Hath it also an intrinsical quality to go from the earth? It cannot be. The cause therefore must be extrinsical. And because when they are come together in the air, if you leave them to their own nature, they will fall down together, they must also

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have some like extrinsical cause. And so this magnetic virtue will be such another virtue as makes all heavy bodies to descend, in this our world, to the earth. If therefore you can from this your hypothesis of compounded motion, by which you have so probably salved the problem of gravity, salve also this of the loadstone, I shall acknowledge both your hypothesis to be true, and your conclusion to be well deduced.

B. I think it not impossible. But I will proceed no farther in it now, than, for the facilitating of the demonstrations, to tell you the several proprieties of the magnet, whereof I am to show the causes. As first, that iron, and no other body, at some little distance, though heavy, will rise to it. Secondly, that if it be laid upon a still water in a floating vessel, and left to itself, it will turn itself till it lie in a meridian, that is to say, with one and the same line still north and south. Thirdly, if you take a long slender piece of iron, and apply the loadstone to it, and, according to the position of the poles of the loadstone, draw it over to the end of the iron, the iron will have the same poles with the magnet, so it be drawn with some pressure; but the poles will lie in a contrary position; and also this long iron will draw other iron to it as the magnet doth.

Fourthly, this long iron, if it be so small as that poised upon a pin, the weight of it have no visible effect, the navigators use it for the needle of their compass, because it points north and south; saving that in most places by particular accidents it is diverted; which diversion is called the variation of the horizontal needle. Fifthly, the same needle placed in a plane perpendicular to the horizon, hath

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Fig. 4.

another motion called the inclination. Which that you may the better conceive, draw a fourth figure; wherein let there be a circle to represent the terrella, that is to say, a spherical magnet.

A. Let this be it, whose centre is A, the north pole B, the south pole C.

B. Join B C, and cross it at right angles with the diameter D E.

A. It is done.

B. Upon the point D set the needle parallel to B C, with the cross of the south pole, and the barb for the north; and describe a square about the circle B D C E, and divide the arch D B into four equal parts in *a, b, c.*

A. It is done.

B. Then place the middle of the needle on the points *a, b, c,* so that they may freely turn; and set the barb which is at D towards the north, and that which is at C towards the south. You see plainly by this, that the angles of inclination through the arch D C taken altogether, are double to a right angle. For when the south point of the needle, looking north, as at D, comes to look south, as at C, it must make half a circle.

A. That is true. And if you draw the sine of the arch D *a,* which is *d a,* and the sine of the arch B *a,* which is *a e,* and the sine of the arch D *b,* which is *b f,* and the sine of the arch B *c,* which is *c g,* the needle will lie upon *b f* with the north-point downwards, so that the needle will be parallel to A D. Then from *a* draw the line *a h,* making the angle *e a h* equal to the angle D A *a.* And then the needle at *a* shall lie in the line *a h* with the south point toward *h.* Finally, draw

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the line *c h*, which, with *c g*, will also make a quarter of a right angle; and therefore if the needle be placed on the point *c*, it will lie in *c h* with the south point toward *h*. And thus you see by what degrees the needle inclines or dips under the horizon more and more from D till it come to the north pole at B; where it will lie parallel to the needle in D; but with their barbs looking contrary ways. And this is certain by experience, and by none contradicted.

You see then why the degrees of the inclinatory needle, in coming from D to B, are double to the degrees of a quadrant. It is found also by experience, that iron both of the mine and of the furnace put into a vessel so as to float, will lay itself (if some accident in the earth hinder it not) exactly north and south. And now I am, from this compounded motion supposed by Copernicus, to derive the causes why a loadstone draws iron; why it makes iron to do the same; why naturally it placeth itself in a parallel to the axis of the earth; why by passing it over the needle it changes its poles; and what is the cause that it inclines. But it is your part to remember what I told you of motion at our second meeting; and what I told you of this compounded motion supposed by Copernicus, at our fourth meeting.

CHAPTER IX.

OF THE LOADSTONE AND ITS POLES, AND WHETHER THEY SHOW THE LONGITUDE OF PLACES ON THE EARTH.

CHAP. IX.

Of the loadstone and its poles, and whether they show the longitude of places on the earth.

A. I COME now to hear what natural causes you can assign of the virtues of the magnet; and first, why it draws iron to it, and only iron.

B. You know I have no other cause to assign but some local motion, and that I never approved of any argument drawn from sympathy, influence, substantial forms, or incorporeal effluvia. For I am not, nor am accounted by my antagonists for a witch. But to answer this question, I should describe the globe of the earth greater than it is at B in the first figure, but that the terrella in the fourth figure will serve our turn. For it is but calling B and C the poles of the earth, and D E the diameter of the equinoctial circle, and making D the east, and E the west. And then you must remember that the annual motion of the earth is from west to east, and compounded of a straight and circular motion, so as that every point of it shall describe a small circle from west to east, as is done by the whole globe. And let the circles about *a b c* be three of those small circles.

A. Before you go any farther, I pray you show me how I must distinguish east and west in every part of this figure. For wheresoever I am on earth, suppose at London, and see the sun rise suppose in Cancer, is not a straight line from my eye to the sun terminated in the east?

B. It is not due east, but partly east, partly south. For the earth, being but a point compared to the sun, all the parallels to D E the equator, such as are $e\,a$, $f\,b$, $c\,g$, if they be produced, will fall upon the body of the sun. And therefore A b is north-east; A a east north-east; and A c north north-east.

A. Proceed now to the cause of attraction.

B. Suppose now that the internal parts of the loadstone had the same motion with that of the internal parts of the sun which make the annual motion of the earth from west to east, but in a contrary way, for otherwise the loadstone and the iron can never be made to meet. Then set the loadstone at a little distance from the earth, marked with z; and the iron marked with x upon the superficies of the earth. Now that which makes x rise to z, can be nothing else but air; for nothing touches it but air. And that which makes the air to rise, can be nothing but those small circles made by the parts of the earth, such are at $a\;b\;c$, for nothing else touches the air. Seeing then the motion of each point of the loadstone is from east to west in circles, and the motion of each point of the iron from west to east; it follows, that the air between the loadstone and the iron shall be cast off both east and west; and consequently the place left empty, if the iron did not rise up and fill it. Thus you see the cause that maketh the loadstone and the iron to meet.

A. Hitherto I assent. But why they should meet when some heterogeneous body lies in the air between them, I cannot imagine. And yet I have seen a knife, though within the sheath, attract one end of the needle of a mariner's compass; and have heard it will do the same though a stone-wall were between.

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B. Such iron were indeed a very vigorous loadstone. But the cause of it is the same that causeth fire or hot water, which have the same compounded motion, to work through a vessel of brass. For though the motion be altered by restraint within the heterogeneous body, yet being continued quite through, it restores itself.

A. What is the cause why the iron rubbed over by a loadstone will receive the virtue which the loadstone hath of drawing iron to it?

B. Since the motion that brings two bodies to meet must have contrary ways, and that the motions of the internal parts of the magnet and of the iron are contrary; the rubbing of them together does not give the iron the first endeavour to rise, but multiplies it. For the iron untouched will rise to a loadstone; but if touched, it becomes a loadstone to other iron. For when they touch a piece of iron, they pass the loadstone over it only one way, viz. from pole to pole; not back again, for that would undo what before had been done; also they press it in passing to the very end of the iron, and somewhat hard. So that by this pressing motion all the small circles about the points *a b c*, are turned the contrary way; and the halves of those small circles made on the arch D B will be taken away and the poles changed, so as that the north poles shall point south, and the south poles north, as in the figure.

A. But how comes it to pass, that when a loadstone hath drawn a piece of iron, you may add to it another, as if they begat one another? Is there the like motion in the generation of animals?

B. I have told you that iron of itself will rise to

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the loadstone; much more then will it adhere to it when it is armed with iron, and both it and the iron have a plain superficies. For then not only the points of contact will be many, which make the coherence stronger, but also the iron wherewith it is armed is now another loadstone, differing a little, which you perhaps think, as male and female. But whether this compounded motion and confrication causeth the generation of animals, how should I know, that never had so much leisure as to make any observation which might conduce to that?

A. My next question is, seeing you say the loadstone, or a needle touched with it, naturally respecteth the poles of the earth, but that the variation of it proceedeth from some accidents in the superficies of the earth; what are those accidents?

B. Suppose there be a hill upon the earth, for example, at *r*; then the stream of the air which which was between *z* and *x* westward, coming to the hill, shall go up the hill's side, and so down to the other side, according to the crooked line which I have marked about the hill by points; and this infallibly will turn the north point of the needle, being on the east side, more towards the east, and that on the other side more towards the west, than if there had been no hill. And where upon the earth are there not eminences and depressions, except in some wide sea, and a great way from land.

A. But if that be true, the variation in the same place should be always the same, for the hills are not removed.

B. The variation of the needle at the same place is still the same; but the variation of the variation

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is partly from the motion of the pole itself, which by the astronomers is called *motus trepidationis;* and partly from that, that the variation cannot be truly observed, for the horizontal needle and the inclinatory needle incline alike, but cannot incline in due quantity. For whether set upon a pin or an axis, their inclination is hindered, in the horizontal needle, by the pin itself: if upon an axis, if the axis be just, it cannot move; if slack, the weight will hinder it; but chiefly because the north pole of the earth draws away from it the north pole of the needle, for two like poles cannot come together. And this is the cause why the variation in one place is east, and another west.

A. This is indeed the most probable reason why the variation varies that ever I heard given; and I should presently acknowledge that this parallel motion of the axis of the earth in the ecliptic, supposed by Copernicus, is the true annual motion of the earth, but that there is lately come forth a book called *Longitude Found,* which makes the magnetical poles distant from the poles of the earth eight degrees and a half.

B. I have the book. It is far from being demonstrated, as you shall find, if you have the patience to see it examined. For wheresoever his demonstration is true, the conclusion, if rightly inferred, will be this, that the poles of the loadstone and the poles of the earth are the same. And where, on the contrary, his demonstrations are fallacies, it is because sometimes he fancieth the lines he hath drawn, not where they are; sometimes because he mistakes his station; and sometimes because he goes on some false principle of

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natural philosophy; and sometimes also because he knoweth not sufficiently the doctrine of spherical triangles.

A. I think that is the book there which lies at your elbow. Pray you read.

B. I find first (p. 4), that the grounds of his argument are the two observations made by Mr. Burroughs, one at Vaygates, in 1576, where the variation from the pole of the earth he found to be 11 deg. 15 min. east; the other at Limehouse, near London, in 1580, where the variation from the pole of the earth was 8 deg. 38 min. west, by which, he saith, he might *find out the magnetical pole*.

A. Where is Vaygates?

B. In 70 degrees of north latitude; the difference of longitude between London and it being 58 degrees.

A. The longitude of places being yet to seek, how came he to know this difference of 58 degrees, except the poles of the magnet and the earth be the same?

B. I believe he trusted to the globe for that. For the distance between the places is not above 2000 miles the nearest way. But we will pass by that, and come to his demonstration, and to his diagram, wherein L is London, P the north-pole of the earth, V Vaygates. So that L P is 38 deg. 28 min.; P V 20 deg.: the angle L P V 58 deg. for the difference between the longitudes of Vaygates and London. This is the construction. But before I come to the demonstration, I have an inference to draw from these observations, which is this. Because in the same year the variation at London

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was 11 deg. 15 min. east, and at Vaygates 8 deg. 38 min. west; if you subtract 11 deg. 15 min. from the arc L P; and 8 deg. 38 min. from the arc L V, the variation on both sides will be taken away; so that P V being the meridian of Vaygates, and L P the meridian of London, they shall both of them meet in P the pole of the earth. And if the pole of the magnet be nearer to the zenith of London than is the pole of the earth, it shall be just as much nearer to the zenith of Vaygates in the meridian of Vaygates, which is P V; as is manifest by the diurnal motion of the earth.

A. All this I conceive without difficulty. Proceed to the demonstration.

B. Mark well now. His words are these (page 5): From P L V subtract 11 deg. 15 min., and there remains the angle V L M. Consider now which is the angle P L V, and which is the remaining angle V L M, and tell what you understand by it.

A. He has marked the angle P L V with two numbers, 11 deg. 15 min. and 21 deg. 50 min., which together make 33 deg. 5 min. And the angle 11 deg. 15 min. being subtracted from P L V, there will remain 21 deg. 50 min. for the angle V L M. I know not what to say to it. For I thought the arc P V, which is 20 deg., had been the arc of the spherical angle P L V; and that the arc L V had been 58 deg., because he says the angle L P V is so; and that the arc L M had been 46 deg., because the angle L P M is so; and lastly, that the angle P L M had been 8 deg. 30 min., because the arc P M is so.

B. And what you thought had been true, if a spherical angle were a very angle. For all men

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that have written of spherical triangles tak the ground of their calculation, as Regiomont Copernicus, and Clavius, that the arch of a sp cal angle is the side opposite to the angle. should have considered also that he make angle V P M 12 deg., but sets down no arc t swer it. But that you may find I am in the r look into the definitions which Clavius hath down before his treatise of spherical triangles amongst them is this; "the arc of a spheric angle is a part of a great circle intercepte tween the two sides drawn from the pole o said great circle."

A. The book is nothing worth; for it is imp ble to subtract an arc of a circle out of a sph angle. And I see besides that he takes the s ficies that lieth between the sides L P and L an arch, which is the quantity of an angle; a a line, and cannot be taken out of a superf I wonder how any man that pretends to m matics could be so much mistaken.

B. It is no great wonder. For Clavius hi striving to maintain that a right angle is greater the angle made by the diameter and the circu ence, fell into the same error. A corner, in v speech, and an angle, in the language of geom are not the same thing. But it is easy even learned man sometimes to take them for the s as this author now has done; and proceedin saith, subtract 8 deg. 38 min. from the P V L, and there remains the angle L V M.

A. That again is false, because impossible. was it that deceived him now?

B. The same misunderstanding of the natu

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a spherical angle. Which appears farther in this, that when he knew the arc V P was part of a great circle, he thought V M, which he maketh 8 deg. 30 min., were also parts of a great circle; which is manifestly false. For two great circles, because they pass through the centre, do cut each other into halves. But V P is not half a circle. He sure thought himself at Vaygates, and that P M V was equal to P V, although in the same hemisphere.

A. But how proves he that the arc P M is 8 degrees 30 minutes.?

B. Thus. We have in two triangles, P L M and P V M, two sides and one angle included, to find P M the distance of the magnetical pole from the pole of the earth 8 deg. 30 min.

A. Is that all? It is very short for a demonstration of two so difficult problems, as the quantity of 8 deg. 30 min.; and of the place of the magnetical pole. But he has proved nothing till he has showed how he found it. And though P M be 8 deg. 30 min., it follows not that M is the magnetical pole.

B. Nor is it true. For if P M be 8 deg. 30 min., and V M 8 deg. 38 min., the whole arc P M V will be 17 deg. 8 min., which should be 20 deg. Besides, whereas the variations were east and west, the subtracting of them should be also east and west, but they are north and south.

A. I am satisfied that the magnetical poles and the poles of the earth are the same. But thus much I confess, if they were not the same, the longitude were found. For the difference of the latitudes of the earth's equator and of the magnetical equator, is the difference of the longitude. But proceed.

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B. "The earth being a solid body, and the magnetic sphere that encompasseth the earth being a substance that hath not solidity to keep pace with the earth, loseth in its motion: and that may be the cause of the motion of the magnetic poles from east to west."

A. This is very fine and unexpected. The magnetic sphere, which I took for a globe made of a magnet, has not solidity to keep pace with the earth, though it be one of the hardest stones that are. It encompasseth the earth; yet I thought nothing had encompassed the earth but air in which I breath and move. By this also the whole earth must be a loadstone. For two bodies cannot be in one place. So that he is yet no farther than Dr. Gilbert whom he slights. And if the sphere be a magnet, then the earth and loadstone have the same poles. See the force of truth! which though it could not draw to it his reason, hath drawn his words to it.

B. But perhaps he meant that the magnetic virtue encompasseth the earth, and not the magnetic body.

A. But that helpeth him not. For if the body of the magnet be not there, the virtue then is the virtue of the earth; and so again the poles of the earth are magnetic poles.

B. You see how unsafe it is to boast of doctrines as of God's gifts, till we are sure that they are true. For God giveth and denieth as he pleaseth, not as ourselves wish; as now to him he hath given confidence enough, but hath denied him, at least hitherto, the finding of the longitudes. In the next place (p. 8) he seems much pleased that

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his doctrine agrees with an opinion of Keplerus, that from the creation to the year of our Lord, it is to the year 1657 now 5650 years; and with that which he saith some divines have held in times past, that as this world was created in six days, so it should continue six thousand years. By which account the world will be at an end three hundred and fifty years hence; though the Scripture tells us it shall come as a thief in the night. O what advantage three hundred and forty years hence will they have that know this, over them that know it not, by taking up money at interest, or selling lands at twenty years' purchase!

A. But he says he will not meddle with that.

B. Yes, when he had meddled with it too much already.

A. But you have not told me wherein consisteth this agreement between him and Keplerus.

B. I forgot it. It is in the motion of the magnetic poles. For precedently (p. 7), he had said "that their period or revolution was six hundred years; their yearly motion thirty-six minutes; and (p. 8) that their motion is by sixes. Six tenths of a degree in one year; six degrees in ten years; sixty degrees in a hundred years; and six times sixty degrees in six hundred years."

A. But what natural cause doth he assign of this revolution of six hundred years?

B. None at all. For the magnet lying upon the earth, can have no motion at all but what the earth and the air give it. And because it is always at 8 deg. 30 min. distance from the pole of the earth, the earth can give it no other motion than what it gives to its own poles by the precession of the

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equinoctial points. Nor can the air give it any motion but by its stream; which must needs vary when the stream varieth. But what a vast difference does he make between the period of the motion of the equinoctial points, which is about or near thirty-six thousand years according to Copernicus (lib. iii. cap. 6), which makes the annual precession to be 36 seconds, and the period of the magnetical poles' motion, which is but six hundred years.

A. Go on.

B. He comes now (p. 15) to the inclinatory needle upon a spherical loadstone. Where he shows, by diagram, that the needle and the instrument together moved towards the magnetical pole, make the sum of the inclinations equal to two quadrants, setting the north-point of the needle southward: which I confess is true. But, in the same page, he ascribeth the same motion to the earth in these words: "as the horizontal needle hath a double motion about the round loadstone or terrella, so also the inclinatory needle hath a double motion about the earth." What is this, but a confession that the poles of the magnet and of the earth are the same?

A. It is plain enough.

B. Besides, seeing he placeth the magnetical pole at M in the meridian of Vaygates, the needle being touched shall incline to the pole of the earth which is P, as well there as at London, and make the north-pole of the earth point south.

A. It is certain, because he puts both the magnetical pole and the pole of the earth in the same meridian of the earth. Nor see I any cause why, the needle being the same, it should not be

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as subject to variation, and to variation of variation, and to all accidents of the earth there, as in any other part.

B. He putteth (p. 16) a question, "at what distance from the earth are the magnetic poles? and answers to it, they are very near the earth, because the nearer the earth, the greater the strength." What think you of this?

A. I think they are in the superficies of the magnet, as the pole of the earth is in the superficies of the earth. And consequently, that then the earth must be a part of the magnet, and their poles the same. For the body of the magnet and the body of the earth, if they be two, cannot be in one place.

B. His next words are, "some things are to be considered concerning those variations of the horizontal needle which are not according to the situation of the place from the magnetic poles, but are contrary; as all the West Indies according to the poles should be easterly, and they are westerly. Which is by some accidental cause in the earth; and their motion, as I formerly said, is a forced motion, and not natural."

A. He has clearly overthrown his main doctrine. For to say the motion of the needle is forced and unnatural, is a most pitiful shift, and manifestly false, no motion being more constant or less accidental, notwithstanding the variation, to which the inclinatory needle is no less subject than the horizontal needle.

B. That which deceived him, was, that he thought them two sorts of needles, forgetting what he had said of Norman's invention of the inclinatory needle by the inclining of the horizontal needle (p. 11).

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For I will show you that what he says is easterly and should be westerly, should be easterly as it is. Consider the fourth figure, in which B is the north-pole, and B *c* 11 deg. 15 min. easterly, which was the variation at London in 1576 easterly. Suppose A *c* to be the needle, shall it not incline, as well here as at D *a*, and the variation B *c* be easterly? Again, D *a* is 11 deg. 15 min., and the needle in D parallel to A B, and at *a* inclining also 11 deg. 15 min. westerly. And is not the variation there D *a* westerly, with the north point of the needle in the line *a h*?

A. But the West-Indies are not in this hemisphere B C D E. The variation therefore will proceed in an arc of the opposite hemisphere, which is westerly.

B. I believe he might think so, forgetting that he and his compass were on the superficies of the earth, and fancying them in the centre at A.

A. It is like enough. If we had a straight line exactly equal to the arc of a quadrant, I think it would very much facilitate the doctrine of spherical triangles.

B. When you have done with your questions of natural philosophy, I will give you a clear demonstration of the equality of a straight line to the arc of a quadrant, which, if it satisfy you, you may carry with you, and try thereby if you can find the angle of a spherical triangle given.

A. It is time now to give over. And at our next meeting I desire your opinion concerning the causes of diaphaniety, and refraction. This Copernicus has done much more than he thought of. For he has not only restored to us astronomy, but also made the way open to physiology.

CHAPTER X.

OF TRANSPARENCE, REFRACTION, AND OF THE POWER OF THE EARTH TO PRODUCE LIVING CREATURES.

CHAP. X.

Of transparence, refraction, and of the power of the earth to produce living creatures.

A. THINKING upon what you said yesterday, it looked like a generation of living creatures. I saw the love between the loadstone and the iron in their mutual attraction, their engendering in their close and contrary motion, and their issue in the iron, which being touched, hath the same attractive virtue. Now seeing they have the same internal motion of parts with that of the earth, why should not their substance be the same, or very near a-kin?

B. The most of them, if not all, that have written on this subject, when they call the loadstone a terrella, seem to think as you do. But I, except I could find proof for it, will not affirm it. For the earth attracteth all kind of bodies but air, and the loadstone none but iron. The earth is a star, and it were too bold to pronounce any sentence of its substance, especially of the planets, that are so lapt up in their several coats, as that they cannot work on our eyes, or any organ of our other senses.

A. I come therefore now to the business of the day. Seeing all generation, augmentation, and alteration is local motion, how can a body not transparent be made transparent?

B. I think it can never be done by the art of man. For as I said of hard and heavy bodies in the creation, so I think of diaphanous, that the very same individual body which was not trans-

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parent then, shall never be made transparent by human art.

A. Do not you see that every day men make glass, and other diaphanous bodies not much inferior in beauty to the fairest gems?

B. It is one thing to make one transparent of many by mixture, and another to make transparent of not transparent. Any very hard stone, if it be beaten into small sands, such as is used for hour-glasses, every one of those sands, if you look upon it with a microscope, you will find to be transparent; and the harder and whiter a stone is, so much the more transparent, as I have seen in the stone of which are made millstones, which stone is here called greet. And I doubt not but the sands of white marble must be more transparent. But there are no sands so transparent that they have not a scurf upon them, as hard, perhaps, as the stone itself; which they whose profession it is to make glass, have the art to scour and wash away. And therefore I think it no great wonder to bring those sands into one lump, though I know not how they do it.

A. I know they do it with lye made with a salt extracted from the ashes of an herb, of which salt they make a strong lye, and mingle it with the sand, and then bake it.

B. Like enough. But still it is a compound of two transparent bodies, whereof one is the natural stone, the other is the mortar. This therefore doth not prove, that one and the same body of not transparent can be made transparent.

A. Since they can make one transparent body of many, why do they not of a great many small

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sparks of natural diamond compound one great one? It would bear the charges of all the materials, and beside, enrich them.

B. It is probable it would. But it may be they know no salt that howsoever prepared, which, with how great a fire soever, can make them melt. And, it may be, the true crystal of the mountain, which is found in great pieces in the Alps, is but a compound of many small ones, and made by the earth's annual motion; for it is a very swift motion. Suppose now that within a very small cavern of those rocks whose smallest atoms are crystal, and the cavity filled with air; and consider what a tumult would be made by the swift reciprocation of that air; whether it would not in time separate those atoms from the rock, and jumbling them together make them rub off their scurf from one another, and by little and little to touch one another in polished planes, and consequently stick together, till in length of time they become one lump of clean crystal.

A. I believe that the least parts of created substances lay mingled together at first, till it pleased God to separate all dissimilar natures, and congregate the similar, to which this annual motion is proper. But they say that crystal is found in the open air hanging like icicles upon the rocks, which, if true, defeats this supposition of a narrow cavern, and therefore I must have some farther experience of it before I make it my opinion. But howsoever, it still holds true that diaphanous bodies of all sorts, in their least parts, were made by God in the beginning of the world. But it may be true, notwithstanding those icicles. For

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the force of the air that could break off those diaphanous atoms in a cavern, can do the same in the open air. And I know that a less force of air can break some bodies into small pieces, not much less hard than crystal, by corrupting them.

B. That which you now have said is somewhat. But I deny not the possibility, but only doubt of the operation. You may therefore pass to some other question.

A. Well, I will ask you then a question about refraction. I know already that for the cause of refraction, when the light falleth through a thinner medium upon a thicker, you assign the resistance of the thicker body; but you do not mean there, by *rarum* and *densum*, two bodies whereof in equal spaces one has more substance in it than the other.

B. No; for equal spaces contain equal bodies. But I mean by *densum* any body which more resisteth the motion of the air, and by *rarum* that which resisteth less.

A. But you have not declared in what that resistance consisteth.

B. I suppose it proceedeth from the hardness.

A. But from thence it will follow, that all transparent bodies that equally refract are equally hard, which I think is not true, because the refraction of glass is not greater, at least in comparison of their hardnesses, than that of water.

B. I confess it. Therefore I think we must take in gravity to a share in the production of this refraction. For I never considered refraction but in glass, because my business then was only to find the causes of the phenomena of telescopes and microscopes. Let therefore A B (in fig. 7) be a

Fig. 7.

Of transparence, refraction, &c.

hard, and consequently, a heavy body; and from above, as from the sun, let C A be the line of incidence, and produced to D; and draw A E perpendicular to A B. It is manifest that the hardness in A B shall turn the stream of the light inwards toward A E, suppose in the line A *e*. It is also evident that the endeavour in B, which is, being heavy, downward, shall turn the stream again inward, towards A E, as in A *b*. Thus it is in refraction from the sun downwards. In like manner, if the light come from below, as from a candle in the point D, the line of incidence will be D A, and produced will pass to C. And the resistance of the hardness in A will turn the stream A C inward, suppose into A *l*, and make C *l* equal to D *e*. For passing into a thinner medium, it will depart from the perpendicular in an angle equal to the angle D A *e*, by which it came nearer to it in A *e*. So also the resistance of the gravity in the point A shall turn the stream of the light into the line A *i*, and make the angle *l* A *i* equal to the angle *e* A *b*. And thus you see in what manner, though not in what proportion, hardness and gravity conjoin their resistance in the causing of refraction.

A. But you proved yesterday, that a heavy body does not gravitate upon a body equally heavy. Now this A B has upper parts and lower parts; and if the upper parts do not gravitate upon the lower parts, how can there be any endeavour at all downward to contribute to the refraction?

B. I told you yesterday, that when a heavy body was set upon another body heavier or harder than itself, the endeavour of it downward was diverted another way, but not that it was extin-

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guished. But in this case, where it lieth upon air, the first endeavour of the lowest part worketh downward. For neither motion nor body can be utterly extinguished by a less than an omnipotent power. All bodies, as long as they are bodies, are in motion one way or other, though the farther it be communicated, so much the less.

A. But since you hold that motion is propagated through all bodies, how hard or heavy soever they be, I see no cause but that all bodies should be transparent.

B. There are divers causes that take away transparency. First, if the body be not perfectly homogeneous, that is to say, if the smallest parts of it be not all precisely of the same nature, or do not so touch one another as to leave no vacuum within it; or though they touch, if they be not as hard in the contact as in any other line. For then the refractions will be so changed both in their direction, and in their strength, as that no light shall come through it to the eye; as in wood and ordinary stone and metal. Secondly, the gravity and hardness may be so great, as to make the angle refracted so great, as the second refraction shall not direct the beam of light to the eye; as if the angle of refraction were D A E, the refracted line would be perpendicular to A B, and never come to the line A D, in which is the eye.

A. To know how much of the refraction is due to the hardness, and how much to the gravity, I believe it is impossible, though the quantity of the whole be easily measured in a diaphanous body given. And both you and Mr. Warner have demonstrated, that as the sine of the angle refracted

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in one inclination is to the sine of the angle refracted in another inclination, so is the sine of one inclination to the sine of the angle of the other inclination. Which demonstrations are both published by Mersennus in the end of the first volume of his *Cogitata Physico-Mathematica.* But since there be many bodies, through which though there pass light enough, yet no object appear through them to the eye, what is the reason of that?

B. You mean paper. For paper windows will enlighten a room, and yet not show the image of an object without the room. But it is because there are in paper abundance of pores, through which the air passing moveth the air within; by the reflections whereof anything within may be seen. And in the same paper there are again as many parts not transparent, through which the air cannot pass, but must be reflected first to all parts of the object, and from them again to the paper; and at the paper either reflected again or transmitted, according as it falls upon pores or not pores; so that the light from the object can never come together at the eye.

A. There belongs yet to this subject the causes of the diversity of colours. But I am so well satisfied with that which you have written of it in the twenty-fourth chapter of your book *de Corpore,* that I need not trouble you farther in it. And now I have but one question more to ask you, which I thought upon last night. I have read in an ancient historian, that living creatures after a great deluge were produced by the earth, which being then very soft, there were bred in it, it may be by the rapid motion of the sun, many blisters, which in time

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breaking, brought forth, like so many eggs, all manner of living creatures great and small, which since it is grown hard it cannot do. What think you of it?

B. It is true that the earth produced the first living creatures of all sorts but man. For God said (Gen. i. 24), *Let the earth produce every living creature, cattle, and creeping thing, &c.* But then again (ver. 25) it is said that *God made the beast of the earth, &c.* So that it is evident that God gave unto the earth that virtue. Which virtue must needs consist in motion, because all generation is motion. But man, though the same day, was made afterward.

A. Why hath not the earth the same virtue now? Is not the sun the same as it was? Or is there no earth now soft enough?

B. Yes. And it may be the earth may yet produce some very small living creatures: and perhaps male and female. For the smallest creatures which we take notice of, do engender, though they do not all by conjunction; therefore if the earth produce living creatures at this day, God did not absolutely rest from all his works on the seventh day, but (as it is chap. ii. 2) *he rested from all the work he had made.* And therefore it is no harm to think that God worketh still, and when and where and what he pleaseth. Beside, it is very hard to believe, that to produce male and female, and all that belongs thereto, as also the several and curious organs of sense and memory, could be the work of anything that had not understanding. From whence, I think we may conclude, that whatsoever was made after the creation, was

Conclusion.

a new creature made by God no otherwise than the first creatures were, excepting only man.

A. They are then in an error that think there are no more different kinds of animals in the world now, than there were in the ark of Noah.

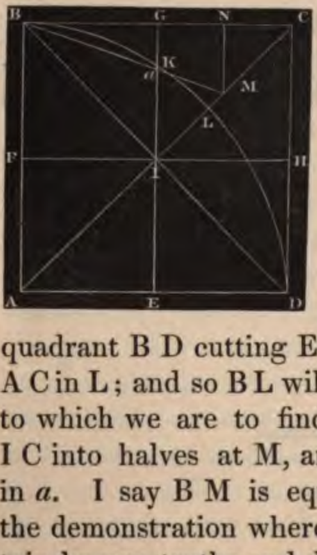
B. Yes, doubtless. For they have no text of Scripture from which it can be proved.

A. The questions of nature which I could yet propound are innumerable. And since I cannot go through them, I must give over somewhere, and why not here? For I have troubled you enough, though I hope you will forgive me.

B. So God forgive us both as we do one another. But forget not to take with you the demonstration of a straight line equal to an arc of a circle.

THE PROPORTION OF A STRAIGHT LINE TO HALF THE ARC OF A QUADRANT.

The proportion of a straight line to half the arc of a quadrant.



Describe the square A B C D, and divide it by the diagonals A C and B D, as also by the straight lines E G, F H, meeting in the centre I at right angles, into four equal parts. Then with the radius A B describe the quadrant B D cutting E G in K, and the diagonal A C in L; and so B L will be half the arc B D, equal to which we are to find a straight line. Divide I C into halves at M, and draw B M cutting E G in *a*. I say B M is equal to the arc B L. For the demonstration whereof we are to assume certain known truths and dictates of common-sense.

1. That the arc B K is the third part of the arc B D, and consequently two-thirds of the arc B L, and B K to K L as two to one.

2. That if a straight line be equal to the arc B L, and one end in B, the other will be somewhere in I C, and higher than the point L.

3. That wheresoever it be, two-thirds of it must

be equal to the arc B K, and one-fifth to the arc K L.

The proportion of a straight line to half the arc of a quadrant.

4. That the arc of a quadrant described in the third part of the radius, or of E G, is equal to the third part of the arc B D, viz. to the arc B K. I may therefore call a third part of E G, the radius of B K; and a sixth part of E G, the radius of the arc K L, &c.

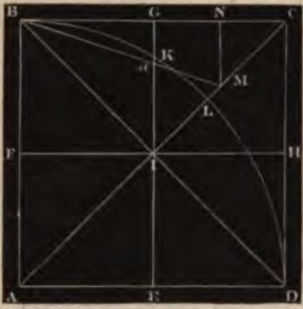
5. And lastly, that any straight line drawn from B to I C, if it be equal to the arc B L, it must cut the half radius I G, whose quadrantal arc is B L, into the proportion of two to one. For as the whole arc to the whole E G, so are the parts of it to the parts of E G.

These premises granted, which I think cannot be denied, I say again, that the straight line B M is equal to the arc B L.

DEMONSTRATION.

Because B I is to I M, by construction, as two to one, and the line I G divides the angle B I C in the midst, B *a* will be to *a* M as two to one, that is to say, as the arc B K to the arc K L. From the point M to the side B C erect a perpendicular M N. And because C M is half C I, the line M N will be half G C; and B N will be three-quarters of B C; and the square of B M equal to ten squares of a quarter of B C; and because B M is to B *a* as three to two, M N will be to *a* G as three to two. But M N is a quarter of E G, therefore *a* G is two-thirds of a quarter of E G; that is, one-third of I G; that is, one-sixth of the whole E G. And I *a* one-third of E G. Therefore I *a* is the radius of the arc B K; and *a* G the radius of the arc

The proportion of a straight line to half the arc of a quadrant.



K L; and E G the radius of the whole arc B L D. Lastly, if a straight line be drawn from B to any other point of the line I C, though any line may be divided into the proportion of two to one, it shall not pass through the point *a*, and therefore not divide the radius of B L, which is I G, into the proportion of two to one. Therefore no straight line can be drawn from B to I C, except B M, so as to be equal to the arc B L. Therefore the straight line B M and the arc B L are equal.

Hence it follows, that seeing the square of B M is equal to ten squares of a quarter of B C, that a straight line equal to the quadrantal arc B L D is equal to ten squares of half the radius, as I have divers ways demonstrated heretofore.

SIX LESSONS

TO THE

PROFESSORS OF THE MATHEMATICS,

ONE OF GEOMETRY, THE OTHER OF ASTRONOMY,

IN THE CHAIRS SET UP BY THE NOBLE AND LEARNED
SIR HENRY SAVILE, IN THE UNIVERSITY
OF OXFORD.

TO THE RIGHT HONOURABLE

HENRY LORD PIERREPONT,

VISCOUNT NEWARK, EARL OF KINGSTON, AND
MARQUIS OF DORCHESTER.

My most noble Lord,

Not knowing on my own part any cause of the favour your Lordship has been pleased to express towards me, unless it be the principles, method, and manners you have observed and approved in my writings; and seeing these have all been very much reprehended by men, to whom the name of public professors hath procured reputation in the university of Oxford, I thought it would be a forfeiture of your Lordship's good opinion, not to justify myself in public also against them, which, whether I have sufficiently performed or not in the six following Lessons addressed to the same professors, I humbly pray your Lordship to consider. The volume itself is too small to be offered to you as a present, but to be brought before you as a controversy it is perhaps the better for being short. Of arts, some are demonstrable, others indemonstrable; and demonstrable are those the construction of the subject whereof is in the power of the artist himself, who, in his demonstration, does no more but deduce the consequences of his own operation. The reason whereof is this, that the science of every subject is derived from a precognition of the causes, generation, and construction of the same; and consequently where the causes are known, there is place for demonstration, but not where the causes are to seek for. Geometry therefore is demonstrable, for the lines and figures from which we reason are drawn and described by ourselves; and civil philosophy is demonstrable, because we make the commonwealth ourselves. But because of natural bodies we know not the construction, but seek it from the effects, there lies no demonstration of what the causes be we seek for, but only of what they may be.

And where there is place for demonstration, if the first principles, that is to say, the definitions contain not the generation of the subject, there can be nothing demonstrated as it ought to be. And this in the three first definitions of Euclid sufficiently appeareth. For seeing he maketh not, nor could make any use of them in his demonstrations, they ought not to be numbered among the principles of geometry. And Sextus Empiricus maketh use of them (misunderstood, yet so understood as the said professors understand them) to the overthrow of that so much renowned evidence of geometry. In that part therefore of my book where I treat of geometry, I thought it necessary in my definitions to express those motions by which lines, superficies, solids, and figures, were drawn and described, little expecting that any professor of geometry should find fault therewith, but on the contrary supposing I might thereby not only avoid the cavils of the sceptics, but also demonstrate divers propositions which on other principles are indemonstrable. And truly, if you shall find those my principles of motion made good, you shall find also that I have added something to that which was formerly extant in geometry.

For first, from the seventh chapter of my book *De Corpore*, to the thirteenth, I have rectified and explained the principles of the science; *id est*, I have done that business for which Dr. Wallis receives the wages. In the seventh, I have exhibited and demonstrated the proportion of the parabola and parabolasters to the parallelograms of the same height and base; which, though some of the propositions were extant without that demonstration, were never before demonstrated, nor are by any other than this method demonstrable.

In the eighteenth, as it is now in English, I have demonstrated, for anything I yet perceive, equation between the crooked line of a parabola or any parabolaster and a straight line.

In the twenty-third I have exhibited the centre of gravity of any sector of a sphere.

Lastly, the twenty-fourth, which is of the nature of refraction and reflection, is almost all new.

But your Lordship will ask me what I have done in the twentieth, about the quadrature of the circle. Truly, my Lord, not much more than before. I have let stand there that which I did before condemn, not that I think it exact, but partly because the division of angles may be more exactly performed by it than by any organical way whatsoever; and I have attempted the same by another method, which seemeth to me very natural, but of calculation difficult and slippery. I call them only aggressions, retaining nevertheless the formal manner of assertion used in demonstration. For I dare not use such a doubtful word as *videtur*, because the professors are presently ready to oppose me with a *videtur quod non*. Nor am I willing to leave those aggressions out, but rather to try if it may be made pass for lawful, (in spite of them that seek honour, not from their own performances, but from other men's failings), amongst many difficult undertakings carried through at once to leave one and the greatest for a time behind; and partly because the method is such as may hereafter give farther light to the finding out of the exact truth.

But the principles of the professors that reprehend these of mine, are some of them so void of sense, that a man at the first hearing, whether geometrician or not geometrician, must abhor them. As for example:

1. That two equal proportions are not double to one of the same proportions.

2. That a proportion is double, triple, &c. of a number, but not of a proportion.

3. That the same body, without adding to it, or

taking from it, is sometimes greater, and sometimes less.

4. That a quantity may grow less and less eternally, so as at last to be equal to another quantity; or, which is all one, that there is a last in eternity.

5. That the nature of an angle consisteth in that which lies between the lines that comprehend the angle in the very point of their concourse, that is to say, an angle is the superficies which lies between the two points which touch, or, as they understand a point, the superficies that lies between the two nothings which touch.

6. That the quotient is the proportion of the division to the dividend.

Upon these and some such other principles is grounded all that Dr. Wallis has said, not only in his *Elenchus* of my geometry, but also in his treatises of the *Angle of Contact*, and in his *Arithmetica Infinitorum;* which two last I have here in two or three leaves wholly and clearly confuted. And I verily believe that since the beginning of the world, there has not been, nor ever shall be, so much absurdity written in geometry, as is to be found in those books of his; with which there is so much presumption joined, that an ἀποκατάϛασις of the like conjunction cannot be expected in less than a Platonic year. The cause whereof I imagine to be this, that he mistook the study of *symbols* for the study of *geometry*, and thought symbolical writing to be a new kind of method, and other men's demonstrations set down in symbols new demonstrations. The way of analysis by squares, cubes, &c., is very ancient, and useful for the finding out whatsoever is contained in the nature and generation of rectangled planes, which also may be found without it, and was at the highest in Vieta; but I never saw anything added thereby to the science of geometry, as being a way wherein men go round from the equality of rectangled planes to the equality of proportion, and thence again to the equality of rectangled planes, wherein the symbols serve only to make men go faster about, as greater wind to a windmill.

It is in sciences as in plants; growth and branching is but the generation of the root continued; nor is the invention of theorems anything else but the knowledge of the construction of the subject prosecuted. The unsoundness of the branches are no prejudice to the roots, nor the faults of theorems to the principles. And active principles will correct false theorems if the reasoning be good; but no logic in the world is good enough to draw evidence out of false or unactive principles. But I detain your Lordship too long. For all this will be much more manifest in the following discourses, wherein I have not only explained and rectified many of the most important principles of geometry, but also by the examples of those errors which have been committed by my reprehenders, made manifest the evil consequence of the principles they now proceed on. So that it is not only my own defence that I here bring before you, but also a positive doctrine concerning the true grounds, or rather atoms of geometry, which I dare only say are very singular, but whether they be very good or not, I submit to your Lordship's judgment. And seeing you have been pleased to bestow so much time, with great success, in the reading of what has been written by other men in all kinds of learning, I humbly pray your Lordship to bestow also a little time upon the reading of these few and short lessons; and if your Lordship find them agreeable to your reason and judgment, let me, notwithstanding the clamour of my adversaries, be continued in your good opinion, and still retain the honour of being,

My most noble Lord,
Your Lordship's most
humble and obliged servant,
THOMAS HOBBES.

LONDON, *June* 10, 1656.

LESSONS

OF

THE PRINCIPLES OF GEOMETRY, &c.

TO THE EGREGIOUS PROFESSORS OF THE MATHEMATICS, ONE OF
GEOMETRY, THE OTHER OF ASTRONOMY, IN THE CHAIRS SET
UP BY THE NOBLE AND LEARNED SIR HENRY SAVILE,
IN THE UNIVERSITY OF OXFORD.

LESSON I.

LESSON I.
Of the principles of geometry, &c.

I SUPPOSE, most egregious professors, you know already that by geometry, though the word import no more but the measuring of land, is understood no less the measuring of all other quantity than that of bodies. And though the definition of geometry serve not for proof, nor enter into any geometrical demonstration, yet for understanding of the principles of the science, and for a rule to judge by, who is a geometrician, and who is not, I hold it necessary to begin therewith.

Geometry is the science of determining the quantity of anything, not measured, by comparing it with some other quantity or quantities measured. Which science therefore whosoever shall go about to teach, must first be able to tell his disciple what measuring or dimension is; by what each several kind of quantity is measured; what quantity is, and what are the several kinds thereof. Therefore as they, who handle any one part of geometry, determine by definition the signification of

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every word which they make the subject or predicate of any theorem they undertake to demonstrate; so must he which intendeth to write a whole body of geometry, define and determine the meaning of whatsoever word belongeth to the whole science. The design of Euclid was to demonstrate the properties of the five regular bodies mentioned by Plato; in which demonstrations there was no need to allege for argument the definition of quantity, which it may be was the cause he hath not anywhere defined it, but done what he undertook without it. And though having perpetually occasion to speak of measure, he hath not defined measure; yet instead thereof he hath, in the beginning of his first elements, assumed an axiom which serveth his turn sufficiently as to the measure of lines, which is the eighth axiom; that those things which lie upon one another all the way (called by him εφαρμόζοντα) are equal. Which axiom is nothing else but a description of the art of measuring length and superficies. For this ἐφάρμοσις can have no place in solid bodies, unless two bodies could at the same time be in one place. But amongst the principles of geometry universal, the definitions are necessary, both of quantity and dimensions.

Quantity is that which is signified by what we answer to him that asketh, *how much* any thing is? and thereby determine the magnitude thereof. For magnitude being a word indefinite, if a man ask of a thing, *quantum est?* that is, *how much* it is, we do not satisfy him by saying it is magnitude or quantity, but by saying it is *tantum, so much*. And they that first called it in Greek,

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πηλικότης, and in Latin *quantity*, might more properly have called it in Latin *tantity*, and in Greek τηλικότης; and we, if we allowed ourselves the eloquence of the Greeks and Latins, should call it the *so-muchness*.

There is therefore to everything concerning which a man may ask without absurdity, *how much it is*, a certain quantity belonging, determining the magnitude to be *so much*. Also wheresoever there is *more* and *less*, there is one kind of quantity or other. And first there is the quantity of bodies, and that of three kinds: length, which is by one way of measuring; superficies, made of the complication of two lengths, or the measure taken two ways; and solid, which is the complication of three lengths, or of the measure taken three ways, for breadth or thickness are but other lengths. And the science of geometry, so far forth as it contemplateth bodies only, is no more but by measuring the length of one or more lines, and by the position of others known in one and the same figure, to determine by ratiocination, how much is the superficies; and by measuring length, breadth, and thickness, to determine the quantity of the whole body. Of this kind of magnitudes and quantities the subject is body.

And because for the computing of the magnitudes of bodies, it is not necessary that the bodies themselves should be present, the ideas and memory of them supplying their presence, we reckon upon those imaginary bodies, which are the quantities themselves, and say the length is so great, the h so great, &c. which in truth is no better say the length is so long, or the breadth so broad, &c. But in the mind of an intelligent man it breedeth no mistake.

Besides the quantity of bodies, there is a quantity of time. For seeing men, without absurdity, do ask how much it is; by answering *tantum, so much*, they make manifest there is a quantity that belongeth unto time, namely, a length. And because length cannot be an accident of time, which is itself an accident, it is the accident of a body; and consequently the length of the time, is the length of the body; by which length or line, we determine how much the time is, supposing some body to be moved over it.

Also we not improperly ask with *how much* swiftness a body is moved; and consequently there is a quantity of motion or swiftness, and the measure of that quantity is also a line. But then again, we must suppose another motion, which determineth the time of the former. Also of force, there is a question of *how much*, which is to be answered by *so much;* that is, by quantity. If the force consist in swiftness, the determination is the same with that of swiftness, namely, by a line; if in swiftness and quantity of body jointly, then by a line and a solid; or if in quantity of body only, as weight, by a solid only.

So also is number quantity; but in no other sense than as a line is quantity divided into equal parts.

Of an angle, which is of two lines whatsoever they be, meeting in one point, the digression or openness in other points, it may be asked how great is that digression? This question is answered also by quantity. An angle therefore hath quantity,

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though it be not the subject of quantity; for the body only can be the subject, in which body those straddling lines are marked.

And because two lines may be made to divaricate by two causes; one, when having one end common and immoveable, they depart one from another at the other ends circularly, and this is called simply an angle; and the quantity thereof is the quantity of the arch, which the two lines intercept.

The other cause is the bending of a straight line into a circular or other crooked line, till it touch the place of the same line, whilst it was straight, in one only point. And this is called an angle of contingence. And because the more it is bent, the more it digresseth from the tangent, it may be asked *how much* more? And therefore the answer must be made by quantity; and consequently an angle of contingence hath its quantity as well as that which is called simply an angle. And in case the digression of two such crooked lines from the tangent be uniform, as in circles, the quantity of their digression may be determined. For, if a straight line be drawn from the point of contact, the digression of the lesser circle will be to the digression of the greater circle, as the part of the line drawn from the point of contact, and intercepted by the circumference of the greater circle is to the part of the same line intercepted by the circumference of the lesser circle, or, which is all one, as the greater radius is to the lesser radius. You may guess by this what will become of that part of your last book, where you handle the question of the quantity of an angle of contingence.

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Also there lieth a question of *how much comparatively* one magnitude is to another magnitude, as how much water is in a tun in respect of the ocean, how much in respect of a pint; *little* in the first respect, *much* in the latter. Therefore the answer must be made by some respective quantity. This respective quantity is called *ratio* and proportion, and is determined by the quantity of their differences; and if their differences be compared also with the quantities themselves that differ, it is called simply proportion, or proportion geometrical. But if the differences be not so compared, then it is called proportion arithmetical. And where the difference is none, there is no quantity of the proportion, which in this case is but a bare comparison.

Also concerning heat, light, and divers other qualities, which have degrees, there lieth a question of *how much*, to be answered by a *so much*, and consequently they have their quantities, though the same with the quantity of swiftness: because the intensions and remissions of such qualities are but the intensions and remissions of the swiftness of that motion by which the agent produceth such a quality. And as quantity may be considered in all the operations of nature, so also doth geometry run quite through the whole body of natural philosophy.

To the principles of geometry the definition appertaineth also of a *measure*, which is this, *one quantity is the measure of another quantity, when it, or the multiple of it, is coincident in all points with the other quantity.* In which definition, instead of that ἐφαρμογὴ of Euclid, I put coincidence.

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For the superposition of quantities, by which they render the word ἐφαρμογὴ, cannot be understood of bodies, but only of lines and superficies. Nevertheless many bodies may be coincident successively with one and the same place, and that place will be their measure, as we see practised continually in the measuring of liquid bodies, which art of measuring may properly be called ἐφάρμοσις, but not superposition.

Also the definitions of *greater*, *less*, and *equal*, are necessary principles of geometry. For it cannot be imagined than any geometrician should demonstrate to us the equality and inequality of magnitudes, except he tell us first what those words do signify. And it is a wonder to me, that Euclid hath not anywhere defined what are equals, or at least, what are equal bodies, but serveth his turn throughout with that forementioned ἐφάρμοσις, which hath no place in solids, nor in time, nor in swiftness, nor in circular, or other crooked lines; and therefore no wonder to me, why geometry hath not proceeded to the calculation neither of crooked lines, nor sufficiently of motion, nor of many other things, that have proportion to one another.

Equal bodies, superficies, and lines, are those of which every one is capable of being coincident with the place of every one of the rest: and equal times, wherein with one and the same motion equal lines are described. And equally swift are those motions by which we run over equal spaces in any time determined by any other motion. And universally all quantities are equal, that are measured by the same number of the same measures.

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It is necessary also to the science of geometry, to define what quantities are of one and the same kind, which they call *homogeneous*, the want of which definitions hath produced those wranglings (which your book *De Angulo Contactus* will not make to cease) about the angle of contingence.

Homogeneous quantities are those which may be compared by (ἐφάρμοσις) application of their measures to one another; so that solids and superficies are heterogeneous quantities, because there is no coincidence or application of those two dimensions. No more is there of line and superficies, nor of line and solid, which are therefore heterogeneous. But lines and lines, superficies and superficies, solids and solids, are homogeneous.

Homogeneous also are line, and the quantity of time; because the quantity of time is measured by the application of a line to a line; for though time be no line, yet the quantity of time is a line, and the length of two times is compared by the length of two lines.

Weight and solid have their quantity homogeneous, because they measure one another by application, to the beam of a balance. Line and angle simply so called, have their quantity homogeneous, because their measure is an arch or arches of a circle applicable in every point to one another.

The quantity of an angle simply so called, and the quantity of an angle of contingence are heterogeneous. For the measures by which two angles simply so called are compared, are in two coincident arches of the same circle; but the measure by which an angle of contingence is measured, is a straight line intercepted between the point of

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contact and the circumference of the circle; and therefore one of them is not applicable to the other; and consequently of these two sorts of angles the quantities are heterogeneous. The quantities of two angles of contingence are homogeneous; for they may be measured by the ἐφάρμοσις of two lines, whereof one extreme is common, namely, the point of contact, the other extremes are in the arches of the two circles.

Besides this knowledge of what is quantity and measure, and their several sorts, it behoveth a geometrician to know why, and of what, they are called principles. For not every proposition that is evident is therefore a principle. A principle is the beginning of something. And because definitions are the beginnings or first propositions of demonstration, they are therefore called principles, principles, I say, of demonstration. But there be also necessary to the teaching of geometry other principles, which are not the beginnings of demonstration, but of construction, commonly called petitions; as that it may be granted *that a man can draw a straight line, and produce it; and with any radius, on any centre describe a circle,* and the like. For that a man may be able to describe a square, he must first be able to draw a straight line; and before he can describe an equilateral triangle, he must be able first to describe a circle. And these petitions are therefore properly called principles, not of demonstration, but of operation. As for the commonly received third sort of principles, called *common notions*, they are principles, only by permission of him that is the disciple; who being ingenuous, and coming not to cavil

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but to learn, is content to receive them,
demonstrable, without their demonstration
though definitions be the only principles
monstration, yet it is not true that every de
is a principle. For a man may so precisel
termine the signification of a word as no
mistaken, yet may his definition be such
never serve for proof of any theorem, n
enter into any demonstration, such as are
the definitions of Euclid, and consequently
no beginnings of demonstration, that is to
principles.

All that hitherto hath been said, is s
and easy to be understood, that you canno
egregious professors, without discovering
norance to all men of reason, though no ge
cians, deny it. And the same (saving t
words are all to be found in dictionarie
also to him that means to learn, not only t
tice, but also the science of geometry ne
and, though it grieve you, mine. And now
to the definitions of Euclid.

The first is of a point: Σημεῖον, &c. "*Sig*
cujus est pars nulla," that is to say, *a mar*
of which there is no part. Which definit
only to a candid, but also to a rigid cons
sound and useful. But to one that neit
interpret candidly, nor can interpret accur
neither useful nor true. Theologers say
hath no part, and that an angel hath no
do not think that soul or angel is a p
mark or as some put instead of it ϛίγμη, w
mark with a hot iron, is visible; if visible
hath quantity, and consequently may be

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into parts innumerable. That which is indivisible is no quantity; and if a point be not quantity, seeing it is neither substance nor quality, it is nothing. And if Euclid had meant it so in his definition, as you pretend he did, he might have defined it more briefly, but ridiculously, thus, *a point is nothing*. Sir Henry Savile was better pleased with the candid interpretation of Proclus, that would have it understood respectively to the matter of geometry. But what meaneth this *respectively to the matter of geometry?* It meaneth this, that no argument in any geometrical demonstration should be taken from the division, quantity, or any part of a point; which is as much as to say, a point is that whose quantity is not drawn into the demonstration of any geometrical conclusion; or, which is all one, whose quantity is not considered.

An accurate interpreter might make good the definition thus, *a point is that which is undivided;* and this is properly the same with *cujus non est pars:* for there is a great difference between *undivided* and *indivisible*, that is, between *cujus non est pars*, and *cujus non potest esse pars*. Division is an act of the understanding; the understanding is therefore that which maketh parts, and there is no part where there is no consideration but of one. And consequently Euclid's definition of a point is accurately true, and the same with mine, which is, that *a point is that body whose quantity is not considered.* And *considered* is that, as I have defined it chap. I. at the end of the third article, which is not put to account in demonstration.

Euclid therefore seemeth not to be of your

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opinion, that say a point is nothing. But why then doth he never use this definition in the demonstration of any proposition? Whether he useth it expressly or no, I remember not; but the sixteenth proposition of the third book without the force of this definition is undemonstrated.

The second definition is of a line: γραμμὴ δὲ μῆκος ἀπλατες. "*Linea est longitudo latitudinis expers; a line is length which hath no breadth;*" and if candidly interpreted, sound enough, though rigorously not so. For to what purpose is it to say *length not broad*, when there is no such thing as a *broad length*. One path may be broader than another path, but not one mile than another mile; and it is not the path but the mile which is the way's length. If therefore a man have any ingenuity he will understand it thus, *that a line is a body whose length is considered without its breadth*, else we must say absurdly a *broad length;* or untruly, that there be bodies which have length and yet no breadth; and this is the very sense which Apollonius, saith Proclus, makes of this definition; "when we measure," says he, "the length of a way, we take not in the breadth or depth, but consider only one dimension." See this of Proclus cited by Sir Henry Savile, where you shall find the very word *consider*.

The fourth definition is of a straight line, thus Ἐυθεῖα γραμμή ἐϛιν, &c. "*Recta linea est quæ ex æquo sua ipsius puncta inter jacet.*" *A straight line is that which lieth equally (or perhaps evenly) between its own points.* This definition is inexcusable. Between what points of its own can a straight line lie but between its extremes? And how lies it

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evenly between them, unless it swerve no more from some other line which hath the same extremes, one way than another? And then why are not between the same points both the lines straight? How bitterly, and with what insipid jests would you have reviled Euclid for this, if living now he had written a *Leviathan*. And yet there is somewhat in this definition to help a man, not only to conceive the nature of a straight line (for who doth not conceive it?) but also to express it. For he meant perhaps to call a straight line that which is all the way from one extreme to another, equally distant from any two or more such lines as being like and equal have the same extremes. So the axis of the earth is all the way equally distant from the circumference of any two or more meridians. But then before he had defined a straight line, he should have defined what lines are *like*, and what are *equal*. But it had been best of all, first to have defined crooked lines, by the possibility of a deduction or setting further asunder of their extremes; and then straight lines, by the impossibility of the same.

The seventh definition, which is that of a plain superficies, hath the same faults.

The eighth is of a plane angle, Ἐπίπεδος γωνία ἐϛὶν ἡ ἐν ἐπιπέδω, &c. "*Angulus planus est duarum linearum in plano se mutuo tangentium, et non in directum jacentium, alterius ad alteram inclinatio.*" *A plane angle is the inclination one towards another of two lines that touch one another in the same plane, and lie not in the same straight line.* Besides the faults here observed by Sir Henry Savile, as the clause of not lying in the same straight line, and the obscurity or equivocation of the word *inclination*, there is yet another, which is, that by this definition two right angles together taken, are no angle; which is a fault which you somewhere (asking leave to use the word *angle*, καταχριστικῶς) acknowledge, but avoid not. For in geometry, where you confess there is required all possible accurateness, every καταχρῆσις is a fault. Besides you see by this definition, that Euclid is not of your, but of Clavius's opinion. For it is manifest that the two lines which contain an angle of contact incline one towards another, and come together in a point, and lie not in the same straight line, and consequently make an angle.

The thirteenth definition is exact, but makes against your doctrine, that a point is nothing. Examine it. Ὅρος ἐστὶν ὅ τινός ἐστῖ πέρας. "*Terminus est quod alicujus extremum est.*" *A term or bound is that which is the extreme of anything.* We had before, *the extremes of a line are points.* But what is in a line the extreme, but the first or last *part*, though you may make that part as small as you will? A point is therefore a part, and nothing is no extreme.

The fourteenth, Σχῆμα ἐστὶ τὸ ὑπὸ τινος ἤ τινῶν ὅρων περιεχόμενον. "*Figura est (subaudi quantitas) quæ ab aliquo, vel aliquibus terminis undique continetur sive clauditur.*" *A figure is quantity contained within some bound or bounds.* Or shortly thus, *a figure is quantity every way determined*, is in my opinion as exact a definition of a figure as can possibly be given, though it must not be so in yours. For this *determination* is the same thing with *circumscription;* and whatsoever is anywhere

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(ubicunque) definitivè is there also *circumscriptivè*; and by this means the distinction is lost, by which theologers, when they deny God to be in any place, save themselves from being accused of saying he is nowhere; for that which is nowhere is nothing. This definition of Euclid cannot therefore possibly be embraced by you that carry double, namely, mathematics and theology. For if you reject it, you will be cast out of all mathematic schools; and if you maintain it, from the society of all school-divines, and lose the thanks of the favour you have shown (you the astronomer) to Bishop Bramhall.

The fifteenth is of a circle. Κοὺκλος ἐϛὶ σχῆμα ἐπίπεδον, &c. *A circle is a plain figure comprehended by one line which is called the circumference, to which circumference all the straight lines drawn from one of the points within the figure are equal to one another.* This is true. But if a man had never seen the generation of a circle by the motion of a compass or other equivalent means, it would have been hard to persuade him that there was any such figure possible. It had been therefore not amiss first to have let him see that such a figure might be described. Therefore so much of geometry is no part of philosophy, which seeketh the proper passions of all things in the generation of the things themselves.

After the fifteenth till the last or thirty-fifth definition, all are most accurate, but the last which is this, *parallel straight lines are those which being in the same plane, though infinitely produced both ways, shall never meet.* Which is less accurate. For how shall a man know that there be straight lines which shall never meet, though both ways infinitely produced? Or how is the definition of parallels, that is, of lines perpetually equidistant, good, wherein the nature of equidistance is not signified? Or if it were signified, why should it not comprehend as well the parallelism of circular and other crooked lines, as of straight, and as well of superficies, as of lines? By parallels is meant equidistant both lines and superficies, and the word is therefore not well defined without defining first equality of distance. And because the distance between two lines or superficies, is the shortest line that can join them, there either ought to be in the definition the *shortest distance*, which is that of the perpendicular and without inclination, or the distance in equal inclination, that is, in equal angles. Therefore if parallels be defined to be those lines or superficies, where the lines drawn from one to another in equal angles be equal, the definition, as to like lines, or like superficies, will be universal and convertible. And if we add to this definition, that the equal angles be drawn not opposite ways, it will be absolute, and convertible in all lines and superficies; and the definition will be this: *parallels are those lines and superficies between which every line drawn, in any angle, is equal to any other line drawn in the same angle the same way*. For by this definition the distance between them will perpetually be equal, and consequently they will never come nearer together, how much, or which way soever they be produced. And the converse of it will be also true, *if two lines, or two superficies be parallel, and a straight line be drawn from one to the other, any other* *straight line, drawn from one to the other in the same angle, and the same way, will be equal to it.* This is manifestly true, and, most egregious professors, new, at least to you.

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And thus much for the definitions placed before the first of Euclid's Elements.

Before the third of his Elements is this definition: "*In circulo æqualiter distare a centro rectæ lineæ dicuntur, cum perpendiculares quæ a centro in ipsas ducuntur sunt æquales.*" *In a circle two straight lines are said to be equally distant from the centre, upon which the perpendiculars drawn from the centre are equal.* This is true; but it is rather an axiom than a definition, as being demonstrable that the perpendicular is the measure of the distance between a point and a straight or a crooked line.

Before the fifth Element the first definition is of a part: *Pars est magnitudo magnitudinis, minor majoris, cum minor metitur majorem. A part is one magnitude of another, the less of the greater, when the less measureth the greater.* From which definition it followeth, that more than a half is not a part of the whole. But because Euclid meaneth here an aliquot part, as a half, a third, or a fourth, &c., it may pass for the definition of a measure under the name of part, as thus: *a measure is a part of the whole, when multiplied it may be equal to the whole,* though properly a measure is external to the thing measured, and not the aliquot part itself, but equal to an aliquot part.

But the third definition is intolerable; it is the definition of λόγος, in Latin *ratio*, in English, *proportion*, in this manner, λόγος ἐϛὶ δύο μεγεθῶν ὁμογενῶν ἡ κατὰ πηλικότητα προς ἄλληλα ποιὰ σχέσις. "*Ratio est duarum magnitudinum ejusdem generis mutua quædam secundum quantitatem habitudo.*" *Proportion is a certain mutual habitude in quantity, of two magnitudes of the same kind, one to another.* First, we have here *ignotum per ignotius;* for every man understandeth better what is meant by *proportion* than by habitude. But it was the phrase of the Greeks when they named like proportions, to say, the first to the second, ὄυτως ἔχει, *id est, ita se habet,* and in English, *is as*, the third to the fourth. As for example, in the proportions of two to four, and three to six, to say two to four, ὄυτως ἔχει, *id est, ita se habet, id est, is as*, three to six. From which phrase Euclid made this his definition of proportion by ποιὰ σχέσις, which the Latins translate *quædam habitudo. Quædam* in a definition is a most certain note of not understanding the word *defined ;* and in Greek, ποιὰ σχέσις is much worse; for to render rightly the Greek definition, we are to say in English, that proportion is a what-shall-I-call-it-*isness*, or *soness* of two magnitudes, &c.; than which nothing can be more unworthy of Euclid. It is as bad as anything was ever said in geometry by Orontius, or by Dr. Wallis. That proportion is quantity compared, that is to say, little or great in respect of some other quantity, as I have above defined it, is I think intelligible.

The fourth is, 'Αναλογία δέ ἐϛιν ἡ των λόγων ὁμοιότης. "*Proportio vero est rationum similitudo.*" Here we have no one word by which to render 'Αναλογία; for our word *proportion* is already bestowed upon the rendering of λόγος. Nevertheless the Greek may be translated into English thus, *iterated propor-*

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tions. But iterated proportion is the same with *eadem ratio.* To what purpose then serveth the sixth definition, which is of *eadem ratio?* For Ἀναλογία and *eadem ratio* and *similitudo rationum,* are the same thing, as appeareth by Euclid himself, where he defines those quantities, that are in the same proportion by ἀνάλογον. Therefore the sixth definition is but a *lemma*, and assumed without demonstration.

The fourteenth, "*Compositio rationis est sumptio antecedentis cum consequente, ceu unius, ad ipsum consequentem,*" *To compound proportion, is to take both antecedent and consequent together as one magnitude, and compare it to the consequent,* is good; though he might have compared it as well with the antecedent; for both ways it had been a composition of proportion. We are to note here, that the composition defined in this place by Euclid is not adding together of proportions, but of two quantities that have proportion. And therefore it is not the same composition which he defineth in the fourth place before the sixth element, for there he defineth the addition of one proportion to another proportion in this manner: λόγος ἐκ λόγων συγκεῖσθαι λέγεται, &c. *A proportion is said to be compounded of proportions, when their quantities multiplied into one another make a proportion;* as when we would compound or add together the proportions of three to two, and of four to five, we must multiply three and four, which maketh twelve, and two and five, which maketh ten. And then the proportion of twelve to ten is the sum of the proportions of three to two, and of four to five, which is true, but not a definition; for it may

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and ought to be demonstrated. For to define what is addition of two proportions (which are always in four quantities, though sometimes one of them be twice named) we are to say, that they are then added together when we make the second to another in the same proportion, which the third hath to the fourth.

And thus much of the definitions; of which some, very few, you see are faulty; the rest either accurate, or good enough if well interpreted. For the rest of the elements all are accurate, notwithstanding that you allow not for good any definition in geometry that hath in it the word *motion*, of which there be divers before the eleventh Element. But I must here put you in mind, that geometry being a science, and all science proceeding from a precognition of causes, the definition of a sphere, and also of a circle, by the generation of it, that is to say, by motion, is better than by the equality of distance from a point within.

The second sort of principles are those of construction, usually called *postulata*, or petitions. As for those *notiones communes*, called *axioms*, they are from the definitions of their terms demonstrable, though they be so evident as they need not demonstration. These petitions are by Euclid called 'Αιτήματα, such as are granted by favour, that is, simply petitions, whereas by axiom is understood that which is claimed as due. So that between 'Αξίωμα and 'Αίτημα there is this other difference, that this latter is simply a petition, the former a petition of right.

Of petitions simply, the first is, *that from any point to any point may be drawn a straight line.*

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The second, *that a finite straight line may be produced*. The third, *that upon any centre at any distance may be described a circle*. All which are both evident and necessary to be granted.

And by all these a man may easily perceive that Euclid in the definitions of a point, a line, and a superficies, did not intend that a point should be nothing, or a line be without latitude, or a superficies without thickness; for if he did, his petitions are not only unreasonable to be granted, but also impossible to be performed. For lines are not drawn but by motion, and motion is of body only. And therefore his meaning was, that the quantity of a point, the breadth of a line, and the thickness of a superficies were not to be *considered*, that is to say, not to be reckoned in the demonstration of any theorems concerning the quantity of bodies, either in length, superficies, or solid.

OF THE FAULTS THAT OCCUR IN DEMONSTRATION.

TO THE SAME EGREGIOUS PROFESSORS OF THE MATHEMATICS IN THE UNIVERSITY OF OXFORD.

LESSON II.

There be but two causes from which can spring an error in the demonstration of any conclusion in any science whatsoever; and those are ignorance or want of understanding, and negligence. For as in the adding together of many and great numbers, he cannot fail that knoweth the rules of addition, and is also all the way so careful, as not

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to mistake one number or one place for another; so in any other science, he that is perfect in the rules of logic, and is so watchful over his pen, as not to put one word for another, can never fail of making a true, though not perhaps the shortest and easiest demonstration.

The rules of demonstration are but of two kinds: one, that the principles be true and evident definitions; the other, that the inferences be necessary. And of true and evident definitions, the best are those which declare the cause or generation of that subject, whereof the proper passions are to be demonstrated. For science is that knowledge which is derived from the comprehension of the cause. But when the cause appeareth not, then may, or rather must we define some known property of the subject, and from thence derive some possible way, or ways, of the generation. And the more ways of generation are explicated, the more easy will be the derivation of the properties; whereof some are more immediate to one, some to another generation. He therefore that proceedeth from untrue, or not understood definitions, is ignorant of that he goes about; which is an ill-favoured fault, be the matter he undertaketh easy or difficult, because he was not forced to undergo a greater charge than he could carry through. But he that from right definitions maketh a false conclusion, erreth through human frailty, as being less awake, more troubled with other thoughts, or more in haste when he was in writing. Such faults, unless they be very frequent, are not attended with shame, as being common to all men, or are at least less ugly than the former, except

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then, when he that committeth them reprehendeth the same in other men. For that is in every man intolerable, which he cannot tolerate in another. But to the end that the faults of both kinds may by every man be well understood, it will not be amiss to examine them by some such demonstrations as are publicly extant. And for this purpose I will take such as are in mine and in your books, and begin with your *Elenchus* of the geometry contained in my book *De Corpore;* to which I will also join your book lately set forth concerning the *Angle of Contact, Conic Sections*, and your *Arithmetica Infinitorum;* and then examine the rest of my philosophy, and yours that oppugn it. For I will take leave to consider you both everywhere as one author, because you publicly declare your approbation of one another's doctrine.

My first definition is of a line, of length, and of a point. "The way," say I, "of a body moved, in which magnitude (though it always have some magnitude) is not considered, is called a line; and the space gone over by that motion, length, or one and a simple dimension." To this definition you say, first, "what mathematician did ever thus define a line or length?" Whether you call in others for help or testimony, it is not done like a geometrician; for they use not to prove their conclusions by witnesses, but rely upon the strength of their own reason; and when your witnesses appear, they will not take your part. Secondly, you grant that what I say is true, but not a definition. But to tell you truly what it is which we call a line, is to define a line. Why then is not this a definition? "Because," say you in the first place, "it

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is not a reciprocal proposition." But by your favour it is reciprocal. For not only the way of a body whose quantity is not considered is a line, but also every line is, or may be conceived to be, the way of a body so moved. And if you object that there is a difference between *is* and *may be conceived to be*, Euclid, whom you call to your aid, will be against you in the fourteenth definition before his eleventh Element; where he defines a sphere just as convertibly as I define a line; except you think the globes of the sun and stars cannot be globes, unless they were made by the circumduction of a semicircle; and again in the eighteenth definition, which is of a cone, unless you admit no figure for a cone, which is not generated by the revolution of a triangle; and again, in the twentieth definition, which is of a cylinder, except it be generated by the circumvolution of a parallelogram. Euclid saw that what proper passion soever should be derived from these his definitions, would be true of any other cylinder, sphere, or cone, though it were otherwise generated; and the description of the generation of any one being by the imagination applicable to all, which is equivalent to convertible, he did not believe that any rational man could be misled by learning logic to be offended with it. Therefore this exception proceedeth from want of understanding, that is, from ignorance of the nature, and use of a definition.

Again, you object and ask: "What need is there of motion, or of body moved, to make a man understand what is a line? Are not lines in a body at rest, as well as in a body moved? And is not the distance of two resting points length, as well

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as the measure of the passage? Is not length one and a simple dimension, and one and a simple dimension line? Why then is not line and length all one?" See how impertinent these questions are. Euclid defines a sphere to be a solid figure described by the revolution of a semicircle about the unmoved diameter. Why do you not ask, what need there is to the understanding of what a sphere is, to bring in the motion of a semicircle? Is not a sphere to be understood without such motion? Is not the figure so made a sphere without this motion? And where he defines the axis of a sphere to be that unmoved diameter, may not you ask, whether there be no axis of a sphere, when the whole sphere, diameter and all, is in motion? But it is not to my purpose to defend my definition by the example of that of Euclid. Therefore first, I say, to me, howsoever it may be to others, it was fit to define a line by motion. For the generation of a line is the motion that describes it. And having defined philosophy in the beginning, to be the knowledge of the properties from the generation, it was fit to define it by its generation. And to your question, *is not distance length?* I answer, that though sometimes distance be equivalent to length, yet certainly the distance between the two ends of a thread wound up into a clue is not the length of the thread; for the length of the thread is equal to all the windings whereof the clue is made. But if you will needs have distance and length to be all one, tell me of what the distance between any two points is the length. Is it not the length of the way? And how is that called way, which is not defined by

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some motion ? And have not several ways between the same places, as by land and by water, several lengths ? But they have but one distance, because the distance is the shortest way. Therefore between the length of the path, and the distance of places, there is a real difference in this case, and in all cases a difference of the consideration. Your objection, that line is longitude, proceeds from want of understanding English. Do men ever ask what is the line of a thread, or the line of a table, or of any other body ? Do they not always ask what is the length of it ? And why, but because they use their own judgments, not yet corrupted by the subtlety of mistaken professors. Euclid defines a line to *be length without breadth.* I those terms be all one, why said he not that a *line is a line without breadth ?* But what definition o a line give you ? None. Be contented then with such as you receive, and with this of mine, which you shall presently see is not amiss.

Your next objections are to my definition of point. Which definition adhereth to the former in these words, "and the body itself is called point." Here again you call for help : "*Qui unquam mortalium, etc.* What mortal man, wha sober man, did ever so define a point ?" It is well and I take it to be an honour to be the first tha do so. But what objection do you bring agains it. This : "That a point added to a point, if i have magnitude, makes it greater." I say it doth so, but then presently it loseth the name of point, which name was given to signify that it wa not the meaning of him that used it in demonstra to add, subtract, multiply, divide, or any wa

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compute it. Then you come in with, "perhaps you will say though it have magnitude, that magnitude is not considered." You need not say *perhaps.* You know I affirm it; and therefore your argument might have been left out, but that it gave you an occasion of a digression into scurvy language.

And whereas you ask why I defined not a point thus: "*Punctum est corpus quod non consideratur esse corpus, et magnum quod non consideratur esse magnum.*" I will tell you why. First, because it is not Latin. Secondly, because when I had defined it by *corpus,* there was no need to define it again by *magnum.* I understand very well this language, "*punctum est corpus, quod non consideratur ut corpus.*" A point is a body not considered as body. But *punctum est corpus, quod non consideratur esse corpus, vel esse magnum,* is not Latin; nor the version of it, *a point is a body which is not considered to be a body,* English. My definition was, that a point is that body whose magnitude is not considered, not reckoned, not put to account in demonstration. And I exemplified the same by the body of the earth describing the ecliptic line; because the magnitude is not there reckoned nor chargeth the ecliptic line with any breadth. But I perceive you understand not what the word *consideration* signifieth, but take it for comparison or relation; and say I ought to define a point simply, and not by relation to a great body; as if to reckon and to compare were the same thing. "*Omnia mihi,*" saith Cicero, "*provisa et considerata sunt.*" I have provided and reckoned everything. There is a great difference between reckoning and relation.

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Again, you ask, why *corpus motum*, a body moved? I will tell you; because the motion was necessary for the generation of a line. And though after the generation of the line the point should rest, yet it is not necessary from this definition that it should be no more a point; nor when Euclid defines a sphere by the circumduction of a semicircle upon an axis that resteth, doth it follow from thence when the sphere, axis, centre and all, as that of the earth, is moved from place to place, that it is no more an axis.

Lastly, you object "that motion is accidentary to a point, and consequently not essential, nor to be put into the definition." And is not the circumduction of a semicircle accidentary to a sphere? Or do you think the sphere of the sun was generated by the revolution of a semicircle? And yet it was thought no fault in Euclid to put the motion into the definition of a sphere.

The conceit you have concerning definitions, that they must explicate the essence of the thing defined, and must consist of a *genus* and a *difference*, is not so universally true as you are made believe, or else there be very many insufficient definitions that pass for good with you in Euclid. You are much deceived if you think these woful notions of yours, and the language that doth everywhere accompany them, show handsomely together. Or that such grounds as these be able to sustain so many, and so haughty reproaches as you advance upon them, so as they fall not, as you shall see immediately, upon your own head. I say [illegible] hath quantity, but not to be reckoned in [illegible] perties of lines, solids, or superficies; you say it hath no quantity at all, but is plainly nothing.

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The first of the petitions of Euclid is, "that a line may be drawn from point to point at any distance." The second, "that a straight line may be produced." The third, "that on any centre a circle may be described at any distance." And the eighth axiom (which Sir H. Savile observes to be the foundation of all geometry) is this, "*Quæ sibi mutuo congruunt, etc.* Those things that are applied to one another in all points are equal." All or any of these principles being taken away, there is not in Euclid one proposition demonstrated or demonstrable. If a point have no quantity, a line can have no latitude; and because a line is not drawn but by motion, by motion of a body, and body imprinteth latitude all the way, it is impossible to draw or produce a straight line, or to describe a circular line without latitude. Also if a line have no latitude, one straight line cannot be applied to another. To them therefore that deny a point to have quantity, that is, a line to have latitude, the forenamed principles are not possible, and consequently no proposition in geometry is demonstrated or demonstrable. You therefore that deny a point to have quantity, and a line to have breadth, have nothing at all of the science of geometry. The practice you may have, but so hath any man that hath learned the bare propositions by heart; but they are not fit to be professors either of geometry or of any other science that dependeth on it. Some man perhaps may say that this controversy is not much worth, and that we both mean the same thing. But that man, though

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in other things prudent enough, knoweth little of science and demonstration. For definitions are not only used to give us the notions of those things whose appellations are defined, for many times they that have no science have the ideas of things more perfect than such as are raised by definitions. As who is there that understandeth not better what a straight line is, or what proportion is, and what many other things are, without definition, than some that set down the definitions of them. But their use is, when they are truly and clearly made, to draw arguments from them for the conclusions to be proved. And therefore you that in your following censures of my geometry, take your argument so often from this, that a point is nothing, and so often revile me for the contrary, are not to be allowed such an excuse. He that is here mistaken, is not to be called negligent in his expression, but ignorant of the science.

In the next place, you take exceptions to my definition of *equal bodies*, which is this: "*Corpora æqualia sunt quæ eundem locum possidere possunt.* Equal bodies are those which may have the same place." To which you object impertinently, that I may as well define a man to be, *he that may be prince of Transylvania*, wittily, as you count wit. Formerly in every definition, you exacted an explication of the essence. You are therefore of opinion that the possibility of being [illegible] Transylvania is no less essential to *a man*, [illegible] lity of the being of two bodies suc- [illegible] same place, is essential to *bodies* [illegible]

[illegible] ice of the twenty-third article

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of this same chapter, where I define what it is we call essence, namely, that accident for which we give the thing its name. As the essence of a man is his capacity of reasoning; the essence of a white body, whiteness, &c., because we give the name of *man* to such bodies as are capable of reasoning, for that their capacity; and the name of *white* to such bodies as have that colour, for that colour. Let us now examine why it is that men say bodies are one to another equal; and thereby we shall be able to determine whether the *possibility of having the same place* be essential or not to *bodies equal*, and consequently whether this definition be so like to the defining of a man by the *possibility of being prince of Transylvania* as you say it is. There is no man, besides such egregious geometricians as yourselves, that inquireth the equality of two bodies, but by measure. And for liquid bodies, or the aggregates of innumerable small bodies, men (men, I say) measure them by putting them one after another into the same vessel, that is to say, into the same place, as Aristotle defines place, or into the space determined by the vessel, as I define place. And the bodies that so fill the vessel, they acknowledge and receive for equal. But though, when hard bodies cannot be so measured, without the incommodity or trouble of altering their figure, they then enquire, if the bodies are both of the same kind, their equality by weight, knowing, without your teaching, that equal bodies of the same nature weigh proportionably to their magnitudes; yet they do it not for fear of missing of the equality, but to avoid inconvenience or trouble. But you (you, I say), that

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have no definition of equals, neither received from others, nor framed by yourselves, out of your shallow meditation and deep conceit of your own wits, contend against the common light of nature. So much is unheedy learning a hinderance to the knowledge of the truth, and changeth into elves those that were beginning to be men.

Again, when men inquire the equality of two bodies in length, they measure them by a common measure; in which measure they consider neither breadth nor thickness, but how the length of it agreeth, first with the length of one of the bodies, then with the length of the other. And both the bodies whose lengths are measured, are successively in the same place under their common measure. *Place* therefore in lines also, is the proper index and discoverer of equality and inequality. And as in length, so it is in breadth and thickness, which are but lengths otherwise taken in the same solid body. But now when we come from this equality and inequality of lengths known by measure, to determine the proportions of superficies and of solids, by ratiocination, then it is that we enter into geometry; for the making of definitions, in whatsoever science they are to be used, is that which we call *philosophia prima*. It is not the work of a geometrician, as a geometrician, to define what is equality, or proportion, or any other word he useth, though it be the work of the same
as a man. His geometrical part is, to draw
em as many true and useful theorems as he

bject secondly, that a pyramis may be
a cube whilst it is a pyramis. True. And

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so also whilst it is a pyramis it hath a possibility by flexion and transposition of parts to become a cube, and to be put into the place where another cube equal to it was before. This is to argue like a child that hath not yet the perfect understanding of any language.

In the third and fourth objection, you teach me to define equal bodies (if I will needs define them by place) by the *equality of place*, and to say, *that bodies are equal that have equal places.* Teach others, if you can, to measure their grain, not by the same, but equal bushels.

In the fifth objection, you except against the the word *can*, in that I say that bodies are equal which *can* fill the same place. For the greater body *can*, you say, fill the place of the less, though not reciprocally the less of the greater. It is true, that though the place of the less can never be the place of the greater, yet it may be filled by a part of the greater. But it is not then the greater body that filleth the place of the less, but a part of it, that is to say, a less body. Howsoever, to take away from simple men this straw they stumble at, I have now put the definition of equal bodies into these words: *equal bodies are those whereof every one can fill the place of every other.* And if my definition displease you, propound your own, either of *equal bodies*, or of *equals* simply. But you have none. Take therefore this of mine.

The sixth is a very admirable exception. "What," say you, "if the same body can sometimes take up a greater, sometimes a lesser place, as by rarefaction and condensation?" I understand very well that bodies may be sometimes thin and sometimes

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thick, as they chance to stand closer together or further from one another. So in the mathematical schools, when you read your learned lectures, you have a thick or thronging audience of disciples, which in a great church would be but a very thin company. I understand how thick and thin may be attributed to bodies in the plural, as to a company; but I understand not how any one of them is thicker in the school than in the church; or how any one of them taketh up a greater room in the school, when he can get in, than in the street. For I conceive the dimensions of the body, and of the place, whether the place be filled with gold or with air, to be coincident and the same; and consequently both the quantity of the air, and the quantity of the gold, to be severally equal to the quantity of the place; and therefore also, by the first axiom of Euclid, equal to one another; insomuch as if the same air should be by condensation contained in a part of the place it had, the dimensions of it would be the same with the dimensions of part of the place, that is, should be less than they were, and by consequence the quantity less. And then either the same body must be less also, or we must make a difference between greater bodies and bodies of greater quantity; which no man doth that hath not lost his wits by trusting them with absurd teachers. When you receive salary, if the steward give you for every shilling a piece of sixpence, and then say, every shilling is condensed into the room of sixpence, I believe you would like this doctrine of yours much the worse. You see how by your ignorance you confound the affairs of mankind, as far forth as they give credit

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to your opinions, though it be but little. For nature abhors even empty words, such as are (in the meaning you assign them), *rarefying* and *condensing*. And you would be as well understood if you should say (coining words by your own power), that the same body might take up sometimes a greater, sometimes a lesser place, by wallifaction and wardensation, as by rarefraction and condensation. You see how admirable this your objection is.

In the seventh objection you bewray another kind of ignorance, which is the ignorance of what are the proper works of the several parts of philosophy. "Though it were out of doubt," say you, "that the same body cannot have several magnitudes, yet seeing it is matter of natural philosophy, nor hath anything to do with the present business, to what purpose is it to mention it in a mathematical definition?" It seems by this, that all this while you think it is a piece of the geometry of Euclid, no less to make the definitions he useth, than to infer from them the theorems he demonstrateth. Which is not true. For he that telleth you in what sense you are to take the appellations of those things which he nameth in his discourse, teacheth you but his language, that afterwards he may teach you his art. But teaching of language is not mathematic, nor logic, nor physic, nor any other science; and therefore to call a definition, as you do, mathematical, or physical, is a mark of ignorance, in a professor inexcusable. All doctrine begins at the understanding of words, and proceeds by reasoning till it conclude in science. He that will learn geometry must un-

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derstand the terms before he begin, which that he may do, the master demonstrateth nothing, but useth his natural prudence only, as all men do when they endeavour to make their meaning clearly known. For words understood are but the seed, and no part of the harvest of philosophy. And this seed was it, which Aristotle went about to sow in his twelve books of *metaphysics*, and in his eight books concerning the hearing of *natural philosophy*. And in these books he defineth time, place, substance or essence, quantity, relation, &c., that from thence might be taken the definitions of the most general words for principles in the several parts of science. So that all definitions proceed from common understanding; of which, if any man rightly write, he may properly call his writing *philosophia prima*, that is, the seeds, or the grounds of philosophy. And this is the method I have used, defining place, magnitude, and the other the most general appellations in that part which I entitle *philosophia prima*. But you now, not understanding this, talk of mathematical definitions. You will say perhaps that others do the same as well as you. It may be so. But the appeaching of others does not make your ignorance the less.

In the eighth place you object not, but ask me *why I define equal bodies apart?* I will tell you. Because all other things which are said to be equal, are said to be so from the equality of bodies; as two lines are said to be equal, when they be coincident with the length of one and the same body; and equal times, which are measured by equal [illegible] body, by the same motion. And the [illegible] there is no subject of quantity, or of equality, or of any other accident but body; all which I thought certainly was evident enough to any uncorrupted judgment; and therefore that I needed first to define equality in the subject thereof, which is body, and then to declare in what sense it was attributed to time, motion, and other things that are not body.

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The ninth objection is an egregious cavil. Having set down the definition of *equal bodies*, I considered that some men might not allow the attribute of equality to any things but those which are the subjects of quantity, because there is no equality, but in respect of quantity. And to speak rigidly, *magnum et magnitudo* are not the same thing; for that which is great, is properly a body, whereof greatness is an accident. In what sense therefore, might you object, can an accident have quantity? For their sakes therefore that have not judgment enough to perceive in what sense men say the length is so long, or the superficies so broad, &c. I added these words: "*Eadem ratione (qua scilicet corpora dicuntur æqualia) magnitudo magnitudini æqualis dicitur,*" that is, *in the same manner, as bodies are said to be equal, their magnitudes also are said to be equal.* Which is no more than to say, *when bodies are equal, their magnitudes also are called equal. When bodies are equal in length, their lengths are also called equal. And when bodies are equal in superficies, their superficies are also called equal.* All which is common speech, as well amongst mathematicians, as amongst common people; and, though improper, cannot be altered, nor needeth to be altered to intelligent men. Nevertheless I did

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think fit to put in that clause, that men might know what it is we call equality, as well in magnitudes as in *magnis*, that is, in bodies. Which you so interpret, as if it bore this sense, *that when bodies are equal their superficies also must be equal*, contrary to your own knowledge, only to take hold of a new occasion of reviling. How unhandsome and unmanly this is, I leave to be judged by any reader that hath had the fortune to see the world, and converse with honest men.

Against the fourteenth article, where I prove that the same body hath always the same magnitude, you object nothing but this, *that though it be granted, that the same body hath the same magnitude, while it resteth, yet I bring nothing to prove that when it changeth place, it may not also change its magnitude by being enlarged or contracted.* There is no doubt but to a body, whether at rest or in motion, more body may be added, or part of it taken away. But then it is not the same body, unless the whole and the part be all one. If the schools had not set your wit awry, you could never have been so stupid as not to see the weakness of such objections. That which you add in the end of your objections to this eighth chapter, *that I allow not Euclid this axiom gratis, that the whole is greater than a part*, you know to be untrue.

At my eleventh chapter, you enter into dispute with me about the nature of proportion. Upon the truth of your doctrine therein, and partly upon the truth of your opinions concerning the definitions of a point, and of a line, dependeth the question whether you have any geometry or none;

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and the truth of all the demonstrations you have in your other books, namely of the *Angle of Contact*, and *Arithmetica Infinitorum*. Here I say you enter, how you will get out, your reputation saved, we shall see hereafter.

When a man asketh what proportion one quantity hath to another, he asketh how great or how little the one is comparatively to, or in respect of the other. When a geometrician prefixeth before his demonstrations a definition, he doth it not as a part of his geometry, but of natural evidence, not to be demonstrated by argument, but to be understood in understanding the language wherein it is set down; though the matter may nevertheless, if besides geometry he have wit, be of some help to his disciple to make him understand it the sooner. But when there is no significant definition prefixed, as in this case, where Euclid's definition of proportion, that it is a *whatshicalt habitude of two quantities, &c.*, is insignificant, and you allege no other, every one that will learn geometry, must gather the definition from observing how the word to be defined is most constantly used in common speech. But in common speech if a man shall ask how much, for example, is six in respect of four, and one man answer that it is greater by two, and another that it is greater by half of four, or by a third of six, he that asked the question will be satisfied by one of them, though perhaps by one of them now, and by the other another time, as being the only man that knoweth why he himself did ask the question. But if a man should answer, as you would do, that the proportion of six to two is of those numbers a certain

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quotient, he would receive but little satisfaction. Between the said answers to this question, how much is six in respect of four? there is this difference. He that answereth that it is more by two, compareth not two with four, nor with six, for two is the name of a quantity absolute. But he that answereth it is more by half of four, or by a third of six, compareth the difference with one of the differing quantities. For halfs and thirds, &c. are names of quantity compared.

From hence there ariseth two species or kinds of (*ratio*) proportion, into which the general word *proportion* may be divided. The one whereof, namely, that wherein the difference is not compared with either of the differing quantities, is called *ratio arithmetica,* arithmetical proportion; the other *ratio geometrica,* geometrical proportion; and, because this latter is only taken notice of by the name of proportion, simply *proportion.* Having considered this, I defined proportion, chapter II. article 3, in this manner: "*Ratio est relatio antecedentis ad consequens secundum magnitudinem:*" *Proportion is the relation of the antecedent to the consequent in magnitude;* having immediately before defined relatives, antecedent, and consequent, in the same article, and by way of explication added, that such relation was nothing else but that one of the quantities was equal to the other, or exceeded it by some quantity, or was by some quantity exceeded by it. And for exemplification of the same, I added further, that the proportion of three to two was, that three exceeded two by a unity; but said not that the unity, or the difference whatsoever it were, was their proportion,

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for unity, and *to exceed another by unity*, is not the same thing. This is clear enough to others; let us therefore see why it is not so to you. You say I make proportion to consist in that which remaineth after the lesser quantity is subtracted out of the greater; and that you make it to consist in the quotient, when one number is divided by the other. Wherein you are mistaken; first, in that you say, I make the proportion to consist in the remainder. For I make it to consist in the act of exceeding, or of being exceeded, or of being equal; whereas the remainder is always an absolute quantity, and never a proportion. To be more or less than another number by two, is not the number two; likewise to be equal to two, where the difference is *nothing*, is not that *nothing?* Again, you mistake in saying the proportion consisteth in the quotient. For divide twenty by five, the quotient is four. Is it not absurd to say that the proportion of five to twenty, or of twenty to five, is four? You may say the proportion of five to twenty, is the proportion of one to four. And so say I. And you may therefore also say, that the proportion of one to four is a measure of the proportion of five to twenty, as being equal. And so say I. But that is only in geometrical proportion, and not in proportion universally. For though the *species* obtain the denomination of the *genus*, yet it is not the *genus*. But as the quotient giveth us a measure of the proportion of the dividend to the divisor in geometrical proportion, so also the remainder after subtraction is the measure of proportion arithmetical.

You object in the next place, "that if the pro-

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portion of one quantity to another be nothing but the excess or defect, then, wheresoever the excess or defect is the same, there the proportion is the same." This you say follows in your logic, and from thence, that the proportion of three to two, and five to four is the same. But is not three to two, and five to four, where the excess is the same number, the same proportion arithmetical? And is not arithmetical proportion, proportion? You take here *(ratio)* proportion, which is the *genus*, for that *species* of it which is called geometrical, because usually this species has the name of proportion simply. Also that the proportion of three to two, is the same with that of nine to six; is it not because the excesses are one and three, the same portions of three and nine, that is to say the same excesses comparatively? I wonder you ask me not what is the *genus* of arithmetical and geometrical proportions, and what the *difference?* The *genus* is *(ratio)* proportion, or comparison in magnitude, and the *difference* is that one comparison is made by the absolute quantity, the other by the comparative quantity, of the excess or defect, if there be any. Can anything be clearer than this? You after come in with *ignosce habitudini* to no purpose. I am not so inhuman as not to pardon dulness or madness: they are not voluntary faults. But when men adventure voluntarily to talk of that they understand not censoriously and scornfully, I may tell them of it.

This difference between the excesses or defects, as they are simply or comparatively reckoned, being thus explained, all the rest of that you say in your objections to this eleventh chapter (saving

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that art. 5 for *ratio binarii ad quinarium est superari ternario*, as it is in other places, I have put too hastily *ratio binarii ad quinarium est ternarius)*, will be understood by every reader to be frivolous, and to proceed from the ignorance of what proportion is.

At the twelfth chapter you only note that I say, *that the proportion of inequality is quantity, but the proportion of equality not quantity*, and refer that which you have to say against it to the chapter following; to which place I shall also come in the following lesson.

OF THE FAULTS THAT OCCUR IN DEMONSTRATION.

TO THE SAME EGREGIOUS PROFESSORS OF THE MATHEMATICS IN THE UNIVERSITY OF OXFORD.

LESSON III.

You begin your reprehension of my thirteenth chapter with a question; whereas *I* divide proportion into arithmetical and geometrical. You ask me what *proportion it is I so divide*. Euclid divides an angle into right, obtuse, and acute. I may ask you as pertinently, what angle it is he so divides? Or, when you divide *animal* into *homo* and *brutum*, what animal that is, which you so divide? You see by this, how absurd your question is. But you say the definition of proportion which I make at Chap. II. art. 3., namely, that proportion is the comparison of two magnitudes, one to another, agreeth not, neither with arithmetical,

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nor with geometrical proportion. I believe you thought so then, but having read what I have said in the end of the last lesson, if you think so still, your fault will be too great to be pardoned easily. But why did you think so before? Is it not because there was no definition in Euclid of proportion universal, and because he maketh no mention of proportion arithmetical, and because you had not in your minds a sufficient notion thereof yourselves to supply that defect? And is not this the cause also, why you put in this parenthesis (if arithmetical proportion ought to be called proportion)? Which is a confession that you know not whether there be such a thing as arithmetical proportion or not, notwithstanding that on all occasions you speak of arithmetical proportionals. Yes, this was it that made you think that proportion universally, and proportion geometrical, is the same, and yet to say you cannot tell whether they be the same or not. It is no wonder, therefore, if in such confusion of the understanding, you apprehend not that the proportions of two to five, and nine to twelve, are the same; so you are blinded by seeing that they are not the same proportions geometrical. Nor doth it help you that I say the difference is the proportion; for by difference you might, if you would, have understood the act of differing.

At the second article you note for a fault in method, that *after I had used the words antecedent and consequent of a proportion in some of the precedent chapters, I define them afterwards.* I do not believe you say this against your knowledge, but that the eagerness of your malice made you

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oversee; therefore go back again to the third article of chapter II. where, having defined correlatives, I add these words, *of which the first is called the* antecedent, *the second the* consequent. This is but an oversight, though such as in me you would not have excused.

At the thirteenth article you find fault with, that I say *that the proportion of inequality, whether it be of excess or of defect, is quantity, but the proportion of equality is not quantity.* Whether that which you say, or that which I say, be the truth, is a question worthy of a very strict examination. The first time I heard it argued, was in Mersennus' chamber at Paris, at such time as the first volume of his *Cogitata Physico-Mathematica* was almost printed; in which, because he had not said all he would say of proportion, he was forced to put the rest into a general preface, which, as was his custom, he did read to his friends before he sent it to the press. In that general preface, under the title *De Rationibus atque Proportionibus*, at the numbers twelve, thirteen, fourteen, he maintaineth against Clavius, *that the composition of proportion is* (as of all other things) *a composition of the parts to make a total,* and *that the proportion of equality answereth in quantity to* non-ens, *or nothing; the proportion of excess, to* ens, *or quantity; and the proportion of defect, to less than nothing; because equality* (he says) *is a term of middle signification between excess and defect.* And there also he refuteth the arguments which Clavius, at the end of the ninth Element of Euclid, bringeth to the contrary. And though this were approved by divers good geometricians then

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present, and never gainsaid by any since, yet do not I say it upon the credit of them, but upon sufficient grounds. For it hath been demonstrated by Eutocius, that *if there be three magnitudes, the proportion of the first to the third is compounded of the proportions of the first to the second, and of the second to the third;* which also I demonstrate in this article. And if there were never so many magnitudes ranked, it might be likewise demonstrated, that the proportion of the first to the last is compounded of the proportions of the first to the second, and of the second to the third, and of the third to the fourth, and so on to the last. If, therefore, we put in order any three numbers, whereof the two last be equal, as four, seven, seven, the proportion of four the first to seven the last, will be compounded of the proportions of four the first to seven the second, and of seven the second to seven the third. Wherefore the proportion of seven to seven (which is of equality) addeth nothing to the proportion of four the first, to seven the second; and consequently the proportion of seven to seven hath no quantity; but that the proportion of inequality hath quantity, I prove it from this, that one inequality may be greater than another.

But for the clearing of this doctrine (which Mersennus calls intricate) of the composition of proportions, I observed, first, that any two quantities, being exposed to sense, their proportion was also exposed; which is not intricate. Again, I observed that if besides the two exposed quantities, there were exposed a third, so as the first were the least, and the third the greatest, or the first the

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greatest, and the third the least, that not only the proportions of the first to the second, but also (because the differences and the quantities proceed the same way) the proportion of the first to the last is exposed by composition, or addition of the differences; nor is there any intricacy in this. But when the first is less than the second, and the second greater than the third, or the first greater than the second, and the second less than the third, so that to make the first and second equal, if we use addition, we must, to make the second and third equal, use subtraction; then comes in the intricacy, which cannot be extricated, but by such as know the truth of this doctrine which I now delivered out of Mersennus, namely, that the proportions of excess, equality, and defect, are as *quantity, not-quantity, nothing want quantity;* or as symbolists mark them $0+1 \,.\, 0 \,.\, 0-1$. And upon this ground I thought depended the universal truth of this proposition, that in any rank of magnitudes of the same kind, the proportion of the first to the last, was compounded of all the proportions (in order) of the intermediate quantities; the want of the proof thereof, Sir Henry Savile calls *(nævus)* a mole in the body of geometry. This proposition is demonstrated at the thirteenth article of this chapter.

But before we come thither, I must examine the arguments you bring to confute this proposition, that the *proportion of inequality is quantity, of equality, not quantity.*

And first, you object that equality and inequality are in the same predicament: a pretty argument to flesh a young scholar in the logic school,

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that but now begins to learn the predicaments. But what do you mean by *æquale* and *inequale?* Do you mean *corpus æquale*, and *corpus inequale?* They are both in the predicament of substance, neither of them in that of quantity. Or do you mean *æqualitas* and *inæqualitas?* They are both in the predicament of relation, neither of them in that of quantity; and yet both *corpus* and *inæqualitas*, though neither of them be quantity, may be *quanta*, that is, both of them have quantity. And when men say body is quantity, or inequality is quantity, they are no otherwise understood, than if they had said *corpus est tantum*, and *inæqualitas tanta,* not *tantitas;* that is, bodies and inequalities are *so much*, not *somuchness;* and all intelligent men are contented with that expression, and yourselves use it. And the quantity of inequality is in the predicament of quantity, because the measure of it is in that line by which one quantity exceeds the other. But when neither exceedeth the other, then there is no line of excess, or defect by which the equality can be measured, or said to be *so much,* or be called quantity. Philosophy teacheth us how to range our words; but Aristotle's ranging them in his predicaments doth not teach philosophy; and therefore no argument taken from thence, can become a doctor and a professor of geometry.

To prove that the proportion of inequality was quantity, but the proportion of equality not quantity, my argument was this: that *because one inequality may be greater or less than another, but one equality cannot be greater nor less than another; therefore inequality hath quantity, or is*

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tanta, *and equality not.* Here you come in again with your predicaments, and object, that to be susceptible of *magis* and *minus*, belongs not to quantity, but to quality; but without any proof, as if you took it for an axiom. But whether true or false, you understand not in what sense it is true or false. It is true that one inequality is inequality, *as well* as another; as one heat is heat *as well* as another, but not *as great*. *Tam*, but not *tantus*. But so it is also in the predicament of quantity; one line is as well a line as another, but not so great. All degrees, intentions, and remissions of quality, are greater or less quantity of force, and measured by lines, superficies, or solid quantity, which are properly in the predicament of quantity. You see how wise a thing it is to argue from the predicaments of Aristotle, which you understand not; and yet you pretend to be less addicted to the authority of Aristotle now than heretofore.

In the next place you say, I may as well conclude from the not susception of *greater* and *less*, that a right angle is not quantity, but an oblique one is. Very learnedly. As if to be *greater* or *less*, could be attributed to a quantity once determined. Number (that is, number indefinitively taken) is susceptible of *greater* and *less*, because one number may be greater than another; and this is a good argument to prove that number is quantity. And do you think the argument the worse for this, that one six cannot be greater than another six? After all these childish arguments which you have hitherto urged, can you persuade any man, or yourselves, that you are logicians?

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To the fifth and sixth article you object, first, *that if I had before sufficiently defined* (ratio) *proportion, I needed not again define what is* (eadem ratio) *the same proportion;* and ask me *whether when I have defined* man, *I use to define anew what is the* same man? You think when you have the definition of *homo,* you have also the definition of *idem homo*, when it is harder to conceive what *idem* signifies, than what *homo*. Besides, *idem* hath not the same signification always, and with whatsoever it be joined; it doth not signify the same with *homo*, that it doth with *ratio*. For with *homo* it signifies the same *individual man*, but with *ratio* it signifies a like, or an equal proportion: and both *(ratio) proportion* and (*idem*) *the same*, being defined, there will still be need of another definition for (*eadem ratio*) *the same proportion*; and this is enough to defend both myself and Euclid, against this objection: for Euclid also, after he had defined (*ratio*) *proportion*, and that sufficiently, as he believed, yet he defines *the same proportion* again apart. I know you did not mean in this place to object anything against Euclid, but you saw not what you were doing. There is within you some special cause of intenebration, which you should do well to look to.

In the next place you say, when I had defined arithmetical proportions to be the same when the difference is the same; it was to be expected I should define geometrical proportions to be then the same, when the antecedents are of their consequents *totuple* or *tantuple*, that is, equimultiple (for *tantuplum* signifies nothing). In plain words, you expected, that as I defined one by subtraction,

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I should define the other by the quotient in division. But why should you expect a definition of the same proportion by the quotient? Neither reason nor the authority of Euclid could move you to expect it. Or why should you say *it was to be expected?* But it seems you have the vanity to place the measure of truth in your own learning. In lines incommensurable there may be the same proportion, when, nevertheless, there is no quotient; for setting their symbols one above another doth not make a quotient: for quotient there is none, but in *aliquot parts*. It is therefore impossible to define proportion universally, by comparing quotients. This incommensurability of magnitudes was it that confounded Euclid in the framing of his definition of proportion at the fifth Element. For when he came to numbers, he defined the *same proportion* irreprehensibly thus: *numbers are then proportional, when the first of the second and the third of the fourth are equimultiple, or the same part, or the same parts;* and yet there is in this definition no mention at all of a quotient. For though it be true, that if in dividing two numbers you make the same quotient, the dividends and the divisors are proportional, yet that is not the definition of the same proportion, but a theorem demonstrable from it. But this definition Euclid could not accommodate to proportion in general, because of incommensurability.

To supply this want, I thought it necessary to seek out some way, whereby the proportion of two lines, commensurable or incommensurable, might be continued perpetually the same. And this I

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found might be done by the proportion of two lines described by some uniform motion, as by an efficient cause both of the said lines, and also of their proportions; which motions continuing, the proportions must needs be all the way the same. And therefore I defined those magnitudes to have the same geometrical proportion, *when some cause producing in equal times equal effects, did determine both the proportions*. This, you say, needs an Œdipus to make it understood. You are, I see, no Œdipus; but I do not see any difficulty, neither in the definition nor in the demonstration. That which you call perplexity in the explication, is your prejudice, arising from the symbols in your fancy. For men that pretend no less to natural philosophy than to geometry, to find fault with bringing motion and time into a definition, when there is no effect in nature which is not produced in time by motion, is a shame. But you swim upon other men's bladders in the superficies of geometry, without being able to endure diving, which is no fault of mine; and therefore I shall, without your leave, be bold to say, I am the first that hath made the grounds of geometry firm and coherent. Whether I have added anything to the edifice or not, I leave to be judged by the readers. You see, you that profess with the pricking of bladders the letting out of their vapour, how much you are deceived. You make them swell more than ever.

For the corollaries that follow this sixth article, you say they contain nothing new. Which is not true. For the ninth is new, and the demonstrations of all the rest are new, being grounded upon a new definition of proportion; and the corollaries

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themselves, for want of a good definition of proportion, were never before exactly demonstrated. For the truth of the sixth definition of the fifth Element of Euclid cannot be known but by this definition of mine; because it requires a trial in all numbers possible, that is to say, an infinite time of trial, whether the quimultiples of the first and third, and of the second and fourth, in all multiplications, do together exceed, together come short, and are together equal; which trial is impossible.

In objecting against the thirteenth and sixteenth article, I observe that you bewray together, both the greatest ignorance and the greatest malice; and it is well, for they are suitable to one another, and fit for one and the same man. In the thirteenth article my proposition is this: *If there be three magnitudes that have proportion one to another, the proportions of the first to the second, and of the second to the third, taken together* (as one proportion), *are equal to the proportion of the first to the third.* This demonstrated, there is taken away one of those moles which Sir Henry Savile complaineth of in the body of geometry. Let us see now what you say, both against the enunciation and against the demonstration.

Against the enunciation you object, *that other men would say* (not the proportions of the first to the second, and of the second to the third, taken together, &c. but) *the proportion which is compounded of the proportion of the first to the second, and of the second to the third*, &c. Is not the compounding of any two things whatsoever the finding of the sum of them both, or the taking of them together as one total? This is that ab-

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surdity of which Mersennus, in the general preface to his *Cogitata Physico-Mathematica*, hath convinced Clavius, who, at the end of Euclid's ninth Element, denieth the composition of proportion to be a composition of parts to make a total; which, therefore, he denied, because he did not observe, that the addition of a proportion of defect to a proportion of excess, was a subtraction of magnitude; and because he understood not that to say, composition is not the making a whole of parts, was contradiction; which all but too learned men would as soon as they heard abhor. Therefore, in saying that other men would not speak in that manner, you say in effect they would speak absurdly. You do well to mark what other geometricians say; but you would do better if you could by your own meditation upon the things themselves, examine the truth of what they say. But you have no mind, you say, to contend about the phrase. Let us see, therefore, what it is you contend about.

The proportion, you say, *which is compounded of double and triple proportion, is not*, as I would have it, *quintuple, but sextuple*, as in these numbers, six, three, one; where the proportion of six to three is double, the proportion of three to one triple, and the proportion of six to one sextuple, not quintuple. Tell me, egregious professors, how is six to three double proportion? Is six to three the double of a number, or the double of some proportion? All men know the number six is double to the number three, and the number three triple to an unity. But is the question here of compounding numbers, or of compounding pro-

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portions? Euclid, at the last proposition of his ninth Element, says indeed, that these numbers, one, two, four, eight, are ἐν διπλασίονι ἀναλογία, in double proportion; yet there is no man that understands it otherwise, than if he had said in proportion of the single quantity to the double quantity; and after the same rate, if he had said three, nine, twenty-seven, &c. had been in triple proportion, all men would have understood it, of the proportion of any quantity to its triple. Your instance, therefore, of six, three, one, is here impertinent, there being in them no doubling, no tripling, no sextupling of proportions, but of numbers. You may observe also, that Euclid never distinguished between double and duplicate, as you do. One word διπλάσιον serves him every where for either. Though, I confess, some curious grammarians take διπλάσιον for duplicate in number, and διπλοῦν for double in quantity; which will not serve your turn. Your geometry is not your own, but you case yourselves with Euclid's; in which, as I have showed you, there be some few great holes; and you by misunderstanding him, as in this place, have made them greater. Though the beasts that think your railing roaring, have for a time admired you; yet now that through these holes of your case I have showed them your ears, they will be less affrighted. But to exemplify the composition of proportions, take these numbers, thirty-two, eight, one, and then you shall see that the proportion of thirty-two to one is the sum of the proportions of thirty-two to eight, and of eight to one. For the proportion of thirty-two to eight is double the proportion of thirty-two to sixteen; and the

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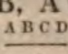
proportion of eight to one, is triple the proportion of thirty-two to sixteen; and the proportion of thirty-two to one is quintuple of thirty-two to sixteen; but double and triple added together maketh quintuple. What can be here denied?

My demonstration consisteth of three cases: the first is when both the proportions are of defect, which is then when the first quantity is the least; as in these three quantities, A B, A C, A D. The first case I demonstrated thus:  Let it be supposed that the point A were moved uniformly through the whole line A D. The proportions, therefore, of A B to A C, and of A C to A D, are determined by the difference of the times in which they are described. And the proportion also of A B to A D, is that which is determined by the difference of the times in which they are described; but the difference of the times in which A B and A C are described, together with the difference of the times wherein A C and A D are described, is the same with the difference of the times wherein are described A B and A D. The same cause, therefore, which determines both the proportions of A B to A C, and of A C to A D, determines also the proportion of A B to A D. Wherefore, by the definition of *the same proportion*, article six, the proportion of A B to A C, together with the proportion of A C to A D, is the same with the proportion of A B to A D.

onsider now your argumentation against it. "*t there be taken*," say you, "*between A and B* *oint* a; and then in your own words, I argue . *The difference of the times wherein are de- d A B and A C, together with the difference*

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of the times wherein are described A C and A D, is the same with the difference of the times in which are described a *B and* a *C (namely, B D, or B C + C D); wherefore, the same cause which determines the two proportions of A B to A C, and of A C to A D, determines also the proportion of* a *B to* a *D.*" Let me ask you here whether you suppose the motion from *a* to B, or from *a* to D, to have the same swiftness with the motion from A to B, or from A to D? If you do not, then you deny the supposition. If you do, then B C, which is the difference of the times A B and A C, cannot be the difference of the times in which are described *a* B and *a* C, except A B and *a* B are equal. Let any man judge now whether there be any paralogism in Orontius that can equal this. And whether all that follows in the rest of this, and the next two whole pages, be not all a kind of raving upon the ignorance of what is the meaning of proportion, which you also make more ill-favoured by writing it; not in language, but in *gambols*; I mean in the symbols, which have made you call those demonstrations short, which put into words so many as a true demonstration requires, would be longer than any of those of Clavius upon the twelfth Element of Euclid.

To the sixteenth article you bring no argument, but fall into a loud *oncethmus* (the special figure wherewith you grace your oratory), offended with my unexpected crossing of the doctrine you teach, that proportion consisteth in a quotient. For that being denied you, your $\frac{a}{b} - \frac{c}{d} + \frac{e}{f} - \frac{g}{h} + \frac{i}{k}$ comes to nothing, that is, to just as much as it is worth. But are not you very simple men, to say that all

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mathematicians speak so, when it is not speaking? When did you see any man but yourselves publish his demonstrations by signs not generally received, except it were not with intention to demonstrate, but to teach the use of signs? Had Pappus no analytics? or wanted he the wit to shorten his reckoning by signs? Or has he not proceeded analytically in a hundred problems (especially in his seventh book), and never used symbols? Symbols are poor unhandsome, though necessary, scaffolds of demonstration; and ought no more to appear in public, than the most deformed necessary business which you do in your chambers. "*But why,*" say you, "*is this limitation to the proportion of the greater to the less?*" I will tell you; because iterating of the proportion of the less to the greater, is a making of the proportion less, and the defect greater. And it is absurd to say that the taking of the same quantity twice should make it less. And thence it is, that in quantities which begin with the less, as one, two, four, the proportion of one to two is greater than that of one to four, as is demonstrated by Euclid, Elem. 5, prop. 8; and by consequent the proportion of one to four, is a proportion of greater littleness than that of one to two. And who is there, that when he knoweth that the respective greatness of four to one, is double to that of the respective greatness of four to two, or of two to one, will not presently acknowledge that the respective greatness of one to two, or two to four, is double to the respective greatness of one to four? But this was too deep for such men as take their opinions, not from weighing, but from reading.

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Lastly you object against the corollary of art. 28; which you make absurd enough by rehearsing it thus: *Si quantitas aliqua divisa supponatur in partes aliquot æquales numero infinitas*, &c. Do you think that of *partes aliquot*, or of *partes aliquotæ*, it can be said without absurdity, that they are *numero infinitæ?* And then you say I seem to mean, that if of the quantity A B, there be supposed a part C B, infinitely little; and that between A C and A B be taken two means, one arithmetical, A E, the other geometrical, A D, the difference between A D and A E, will be infinitely little. My meaning is, and is sufficiently expressed, that the said means taken everywhere (not in one place only) will be the same throughout: and you that say there needed not so much pains to prove it, and think you do it shorter, prove it not at all. For why may not I pretend against your demonstration, that B E, the arithmetical difference, is greater than B D, the geometrical difference. You bring nothing to prove it; and if you suppose it, you suppose the thing you are to prove. Hitherto you have proceeded in such manner with your *Elenchus*, as that so many objections as you have made, so many false propositions you have advanced. Which is a peculiar excellence of yours, that for so great a stipend as you receive, you will give place to no man living for the number and grossness of errors you teach your scholars.

At the fourteenth chapter your first exception is to the second article; where I define a plane in this manner: *A plane superficies is that which is described by a straight line so moved, as that every point thereof describe a several straight line.* In

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which you require, first, that instead of *describe*, I should have said *can describe*. Why do you not require of Euclid, in the definition of a cone, instead of *continetur, is contained*, he say *contineri potest, can be contained?* If I tell you how one plane is generated, cannot you apply the same generation to any other plane? But you object, that the plane of a circle may be generated by the motion of the *radius*, whose every point describeth, not a straight, but a crooked line, wherein you are deceived; for you cannot draw a circle (though you can draw the perimeter of a circle) but in a plane already generated. For the motion of a straight line, whose one point resting, describeth with the other points several perimeters of circles, may as well describe a conic superficies, as a plane. The question, therefore, is, how you will, in your definition, take in the plane which must be generated before you begin to describe your circle, and before you know what point to make your centre. This objection, therefore, is to no purpose; and besides, that it reflecteth upon the perfect definitions of Euclid before the eleventh Element, it cannot make good his definition (which is nothing worth) of a plane superficies, before his first Element.

In the next place, you reprehend briefly this *corollary, that two planes cannot enclose a solid.* I should, indeed, have added, *with the base on whose extremes they insist*: but this is not a fault to be ashamed of; for any man, by his own understanding, might have mended my expression without departing from my meaning. But from your doctrine, *that a superficies has no thickness*, it is

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impossible to include a solid, with any number of planes whatsoever, unless you say that solid is included which nothing at all includes.

At the third article, where I say *of crooked lines, some are everywhere crooked, and some have parts not crooked.* You ask me what crooked line has parts not crooked; and I answer, it is that line which with a straight line makes a rectilineal triangle. But this, you say, cannot stand with what I said before, namely, that a straight and crooked line cannot be coincident; which is true, nor is there any contradiction; for that part of a crooked line which is straight, may with a straight line be coincident.

To the fourth article, where I define *the centre of a circle to be that point of the radius, which in the description of the circle is unmoved;* you object as a contradiction, that I had before defined a point to be the body which is moved in the description of a line: foolishly, as I have already shown at your objection to Chap. VIII. art. 12.

But at the sixth article, where I say, that *crooked and incongruous lines touch one another but in one point,* you make a cavil from this, that *a circle may touch a parabola in two points.* Tell me truly, did you read and understand these words that followed? "*A crooked line cannot be congruent with a straight line; because if it could, one and the same line should be both straight and crooked.* If you did, you could not but understand the sense of my words to be this: *when two crooked lines which are incongruous, or a crooked and a straight line touch one another, the contact is not in a line, but only in one point;* and then

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your instance of a circle and a parabola was a wilful cavil, not befitting a doctor. If you either read them not, or understood them not, it is your own fault. In the rest that followeth upon this article, with your diagram, there is nothing against me, nor anything of use, novelty, subtlety, or learning.

At the seventh article, where I define both an *angle*, simply so called, and an *angle of contingence*, by their several generations; namely, that the former is generated *when two straight lines are coincident, and one of them is moved, and distracted from the other by circular motion upon one common point resting, &c.;* you ask me "*to which of these kinds of angle I refer the angle made by a straight line when it cuts a crooked line?*" I answer easily and truly, To that kind of angle which is called simply an angle. This you understand not. "For how", will you say, "can that angle which is generated by the divergence of two straight lines, be other than rectilineal? or how can that angle which is not comprehended by two straight lines, be other than curvilineal?" I see what it is that troubles you; namely, the same which made you say before, that if the body which describes a line be a point, then there is nothing which is not moved that can be called a point. So you say here, "If an angle be generated by the motion of a straight line, then no angle so generated can be curvilineal;" which is as well argued, as if a man should say, the house was built by the carriage and motion of stone and timber, therefore, when the carriage and that motion is ended, it is no more a house. Rectilineal and curvilineal hath

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nothing to do with the nature of an angle simply so called, though it be essential to an angle of contact. The measure of an angle, simply so called, is a circumference of a circle; and the measure is always the same kind of quantity with the thing measured. The rectitude or curvity of the lines, which drawn from the centre, intercept the arch, is accidentary to the angle, which is the same, whether it be drawn by the motion circular of a straight line or of a crooked. The diameter and the circumference of a circle make a right angle, and the same which is made by the diameter and the tangent. And because the point of contact is not, as you think, nothing, but a line unreckoned, and common both to the tangent and the circumference; the same angle computed in the tangent is rectilineal, but computed in the circumference, not rectilineal, but mixed: or, if two circles cut one another, curvilineal. For every chord maketh the same angle with the circumference which it maketh with the line that toucheth the circumference at the end of the chord. And, therefore, when I divide an angle, simply so called, into rectilineal and curvilineal, I respect no more the generation of it, than when I divide it into right and oblique. I then respect the generation, when I divide an angle into an angle simply so called, and an angle of contact. This that I have now said, if the reader remember when he reads your objections to this, and to the ninth article, he will need no more to make him see that you are utterly ignorant of the nature of an angle; and that if ignorance be madness, not I, but you, are mad: and when an angle is comprehended between a straight

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and a crooked line (if I may compute the same angle as comprehended between the same straight line and the point of contact), that it is consonant to my definition of a point by a *magnitude not considered*. But when you, in your treatise, *De Angulo Contactus* (chap. III. p. 6, l. 8) have these words: "*Though the whole concurrent lines incline to one another, yet they form no angle anywhere but in the very point of concourse*:" you, that deny a point to be anything, tell me how two nothings can form an angle; or if the angle be not formed, neither before the concurrent lines meet, nor in the point of concourse, how can you apprehend that any angle can possibly be framed? But I wonder not at this absurdity; because this whole treatise of yours is but one absurdity, continued from the beginning to the end, as shall then appear when I come to answer your objections to that which I have briefly and fully said of that subject in my 14th chapter.

At the twelfth article, I confess your exception to my universal definition of parallels to be just, though insolently set down. For it is no fault of ignorance (though it also infect the demonstration next it), but of too much security. The definition is this: *Parallels are those lines or superficies, upon which two straight lines falling, and wheresoever they fall, making equal angles with them both, are equal;* which is not, as it stands, universally true. But inserting these words *the same way*, and making it stand thus: *parallel lines or superficies, are those upon which two straight lines falling the same way, and wheresoever they fall, making equal angles. are* [illegible]t is both true

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and universal; and the following consectary, with very little change, as you may see in the translation, perspicuously demonstrated. The same fault occurreth once or twice more; and you triumph unreasonably, as if you had given therein a very great proof of your geometry.

The same was observed also upon this place by one of the prime geometricians of Paris, and noted in a letter to his friend in these words (Chap. XIV. art. 12): "*The definition of parallels wanteth somewhat to be supplied.*" And of the consectary he says, "*It concludeth not, because it is grounded on the definition of parallels.*" Truly and severely enough, though without any such words as savour of arrogance, or of malice, or of the clown.

At the thirteenth article you recite the demonstration by which I prove the perimeters of two circles to be proportional to their semidiameters; and with *esto, fortasse, recte, omnino*, noddying to the several parts thereof, you come at length to my last inference: *Therefore, by* Chap. XIII. art. 6, *the perimeters and semidiameters of circles are proportional;* which you deny; and therefore deny, because you say it followeth by the same ratiocination, that *circles also and spheres are proportional to their semidiameters.* "*For the same distance,* you say, *of the perimeter from the centre which determines the magnitude of the semidiameter, determines also the magnitude both of the circle and of the sphere.*" You acknowledge that perimeters and semidiameters have the cause of their determination such as in equal times make equal spaces. Suppose now a sphere generated by the semidiameters, whilst the semicircle

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is turned about. There is but one *radius* of an infinite number of *radii*, which describes a great circle; all the rest describe lesser circles parallel to it, in one and the same time of revolution. Would you have men believe, that describing greater and lesser circles, is according to the supposition (*temporibus æqualibus æqualia facere*) to make equal spaces in equal times? Or, when by the turning about of the semidiameter is described the plane of a circle, does it, think you, in equal times make the planes of the interior circles equal to the planes of the exterior? Or is the *radius* that describes the inner circles equal to the *radius* that describes the exterior? It does not, therefore, follow from anything I have said in this demonstration, that either spheres or planes of circles, are proportional to their *radii*; and consequently, all that you have said, triumphing in your own incapacity, is said imprudently by yourselves to your own disgrace. They that have applauded you, have reason by this time to doubt of all the rest that follows, and if they can, to dissemble the opinion they had before of your geometry. But they shall see before I have done, that not only your whole *Elenchus*, but also your other books of the *Angle of Contact*, &c. are mere ignorance and gibberish.

To the fourteenth article you object, that (in the sixth figure) I assume gratis, that F G, D E, B C, are proportional to A F, A D, A B; and you refer it to be judged by the reader: and to the reader I refer it also. The not exact drawing of the figure (which is now amended) is it that deceived you. For A F, F D, D B, are equal by construction. Also,

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AG, GE, EC, are equal by construction. And FG, DK, BH, KE, HI, IC, are equal by parallelism. And because AF, FG, are as the velocities wherewith they are described; also 2 AF (that is AD) and 2 FG (that is DE) are as the same velocities. And finally, 3 AF (that is AB) and 3 FG (that is BC) are as the same velocities. It is not therefore assumed gratis, that FG, DE, BC are proportional to AF, AD, AB, but grounded upon the sixth article of the thirteenth chapter; and consequently your objection is nothing worth. You might better have excepted to the placing of DE, first at adventure, and then making AD two-thirds of AB; for that was a fault, though not great enough to trouble a candid reader; yet great enough to be a ground, to a malicious reader, of a cavil.

That which you object to the third *corollary* of art. 15, was certainly a dream. There is no assuming of an angle CDE, for an angle HDE, or BDE, neither in the demonstration, nor in any of the corollaries. It may be you dreamt of somewhat in the twentieth article of chapter XVI. But because that article, though once printed, was afterwards left out, as not serving to the use I had designed it for, I cannot guess what it is: for I have no copy of that article, neither printed nor written; but am very sure, though it were not useful, it was true.

Article the sixteenth. Here we come to the controversy concerning the *angle of contact, which*, you say, *you have handled, in a special treatise published; and that you have clearly demonstrated, in your public lectures, that Peletarius*

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was in the right. But that I agree not sufficiently, neither with Peletarius nor with Clavius. I confess I agree not in all points with Peletarius, nor in all points with Clavius. It does not thence follow that I agree not with the truth. I am not, as you, of any faction, neither in geometry nor in politics. If I think that you, or Peletarius, or Clavius, or Euclid, have erred, or been too obscure, I see no cause for which I ought to dissemble it. And in this same question I am of opinion that Peletarius did not well in denying the *angle of contingence* to be *an angle*. And that Clavius did not well to say, *the angle of a semicircle* was less than *a right-lined right angle.* And that Euclid did not well to leave it so obscure what he meant by *inclination* in the definition of a *plane angle*, seeing elsewhere he attributeth inclination only to acute angles; and scarce any man ever acknowledged inclination in a straight line, to any other line to which it was perpendicular. But you, in this question of what is inclination, though you pretend not to depart from Euclid, are, nevertheless, more obscure than he; and also are contrary to him. For Euclid by inclination meaneth the inclination of one line *to* another; and you understand it of the inclination of one line *from* another; which is not inclination, but declination. For you make two straight lines, when they lie one on another, to lie ἀκλινῶς, that is, without any inclination (because it serves your turn); not observing that it followeth thence, that inclination is a digression of one line *from* another. This is in your first argument in the behalf of Peletarius (p. 10, l. 22), and destroys his opinion. For, according to Eu-

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clid, the greatest angle is the greatest inclination; and an angle equal to two right angles by this ἀκλισία, should not be the greatest inclination, as it is, but the least that can be. But if by the inclination of two lines, we understand that proceeding of them to a common point, which is caused by their generation, which, I believe, was Euclid's meaning; then will the *angle of contact* be no less an *angle* than a *rectilineal angle,* but only (as Clavius truly says it is) heterogeneous to it; and the doctrine of Clavius more conformable to Euclid than that of Peletarius. Besides, if it be granted you, that there is no inclination of the circumference to the tangent, yet it does not follow that their concourse doth not form some kind of angle; for Euclid defineth there but one of the kinds of a plane angle. And then you may as much in vain seek for the proportion of such angle to the angle of contact, as seek for the *focus* or *parameter of the parabola of Dives and Lazarus.* Your first argument therefore is nothing worth, except you make good that which in your second argument you affirm, namely, that all plane angles, not excepting the angle of contact, are *(homogeneous)* of the same kind. You prove it well enough of other curvilineal angles; but when you should prove the same of an angle of contact, you have nothing to say but (p. 17, l. 15), " *Unde autem illa quam somniet heterogenia oriatur, neque potest ille ullatenus ostendere, neque ego vel somniare:*" "*Whence should arise that diversity of kind which he dreams of, neither can he at all show, nor I dream;*" as if you knew what he could do if he were to answer you; or all were false which you

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cannot dream of. So that besides your customary vanity, here is nothing hitherto proved, neither for the opinion of Peletarius, nor against that of Clavius. I have, I think, sufficiently explicated, in the first lesson, that the angle of contact is quantity, namely, that it is the quantity of that crookedness or flexion, by which a straight line is bent into an arch of a circle equal to it; and that because the crookedness of one arch may be greater than the crookedness of another arch of another circle equal to it; therefore the question *quanta est curvitas*, how much is the crookedness, is pertinent, and to be answered by *quantity*. And I have also shown you in the same lesson, that the quantity of one angle of contact is compared with that of another angle of contact by a line drawn from the point of contact, and intercepted by their circumferences; and that it cannot be compared by any measure with a rectilineal angle.

But let us see how you answer to that which Clavius has objected already. "*They are heterogeneous*," says he, "*because the angle of contact, how oft soever multiplied, can never exceed a rectilineal angle.*" To answer which, you allege *it is no angle at all; and that therefore, it is no angle at all, because the lines have no inclination one to another*. How can lines that have no inclination one to another, ever come together? But you answer, *at least they have no inclination in the point of contact*. And why have two straight lines inclination before they come to touch, more than a straight line and an arch of a circle? And in the point of contact itself, how can it be that there is less inclination of the two points of a

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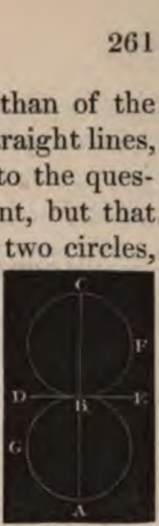
straight line and an arch of a circle, than of the points of two straight lines? But the straight lines, you say, will cut; which is nothing to the question; and yet this also is not so evident, but that it may receive an objection. Suppose two circles, A G B and C F B, to touch in B, and have a common tangent through B. Is not the line C F B G A a crooked line? and is it not cut by the common tangent D B E? What is the quantity of the two angles F B E and G B D, seeing you say neither D B G nor E B F is an angle? It is not, therefore, the cutting of a crooked line, and the touching of it, that distinguisheth an angle simply, from an angle of contact. That which makes them differ, and in kind, is, that the one is the quantity of a *revolution*, and the other, the quantity of *flexion*.



In the seventh chapter of the same treatise, you think you prove the angle of contact, if it be an angle, and a rectilineal angle to be (*homogeneous*) of the same kind; when you prove nothing but that you understand not what you say. Those quantities which can be added together, or subtracted one from another, are of the same kind; but an angle of contact may be subtracted from a right angle, and the remainder will be the angle of a semicircle, &c. So you say, but prove it not, unless you think a man must grant you that the superficies contained between the tangent and the arch, which is it you subtract, is the angle of contact; and that the plane of the semicircle is the angle of the semicircle, which is absurd; though, as absurd as it is, you say it directly in your *Elen-*

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chus, p. 35, l. 14, in these words: "*When Euclid defines a plane angle to be the inclination of two lines, he meaneth not their aggregate, but that which lies between them.*" It is true, he meaneth not the aggregate of the two lines; but that he means that which lies between them, which is nothing else but an indeterminate superficies, is false, or Euclid was as foolish a geometrician as either of you two.

Again, you would prove the angle of contact, if it be an angle, to be of the same kind with a rectilineal angle, out of Euclid (III. 16); where he says, *it is less than any acute angle*. And it follows well, that if it be an angle, and less than any rectilineal angle, it is also of the same kind with it. But, to my understanding, Euclid meant no more, but that it was neither greater nor equal which is as truly said of heterogeneous, as of homogeneous quantities. If he meant otherwise, he confirms the opinion of Clavius against you, or makes the quantity of an angle to be a superficies, and indefinite. But I wonder how you dare venture to determine whether two quantities be homogeneous or not, without some definition of homogeneous (which is a hard word), that men may understand what it meaneth.

In your eighth chapter you have nothing but Sir H. Savile's authority, who had not then resolved what to hold; but esteeming the angle
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can make it equal to the least rectilineal angle, with great ingenuity returneth to his former uncertainty.

In your ninth and tenth chapters you prove with much ado, that the angles of like segments are equal; as if that might not have been taken gratis by Peletarius, without demonstration. And yet your argument, contained in the ninth chapter, is not a demonstration, but a conjectural discourse upon the word *similitude*. And in the eleventh chapter, wherein you answer to an objection, which might be made to your argument in the precedent page, taken from the parallelism of two concentric circles, though objection be of no moment, yet you have in the same treatise of yours that which is much more foolish, which is this, (p. 38, l. 12): "*Non enim magnitudo anguli,*" *&c. The magnitude of an angle is not to be estimated by that straddling of the legs, which it hath without the point of concourse, but by that straddling which it hath in the point of the concourse itself.*" I pray you tell me what straddling there is of two coincident points, especially such points as you say are nothing? When did you ever see two nothings straddle?

The arguments in your twelfth and thirteenth chapters are grounded all on this untruth, that an angle is that which is contained between the lines that make it; that is to say, is a plane superficies, which is manifestly false; because the measure of an angle is an arch of a circle, that is to say, a line; which is no measure of a superficies. Besides this gross ignorance, your way of demonstration, by putting N for a great number of sides of an equi-

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lateral polygon, is not to be admitted; for, thoug you understand something by it, you demonstrate nothing to anybody but those who understand your symbolic tongue, which is a very narrow language. If you had demonstrated it in Irish o Welsh, though I had not read it, yet I should no have blamed you, because you had written to considerable number of mankind, which now you do not.

In your last chapters you defend Vitellio without need; for there is no doubt but that whatsoever crooked line be touched by a straight line, the angle of contingence will neither add anything to, nor take anything from, a rectilineal right angle; but that it is because the angle of contact is no angle, or no quantity, is not true. For it is therefore an angle, because an angle of contact; and therefore quantity, because one angle of contact may be greater than another; and therefore heterogeneal, because the measure of an angle of contact cannot (*congruere*) be applied to the measure of a rectilineal angle, as they think it may, who affirm with you that the nature of an angle consisteth in that which is contained between the lines that comprehend it, viz., in a plane superficies. And thus you see in how few lines, and without brachygraphy, your treatise of the angle of contingence is discovered for the greatest part to be false, and for the rest, nothing but a detection of some errors of Clavius grounded on the same false principles with your own. To return now from your treatise of the angle of contact back again to your *Elenchus*.

The fault you find at art. 18, is, that I under-

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stand not that Euclid makes a *plane angle* to be that which is contained between the two lines that form it. It is true, that I do not understand that Euclid was so absurd, as to think the nature of an angle to consist in superficies; but I understand that you have not had the wit to understand Euclid.

The nineteenth article of mine in this fourteenth chapter, is this: "*All respect or variety of position of two lines, seemeth to be comprehended in four kinds.* For they are either *parallel*, or *(being if need be produced) make an angle;* or, (if drawn out far enough) *touch;* or, lastly, they are *asymptotes*"; in which you are first offended with the word *It seems.* But I allow you, that never err, to be more peremptory than I am. For to me it seemed (I say again seemed) that such a phrase, in case I should leave out something in the enumeration of the several kinds of position, would save me from being censured for untruth; and yet your instance of two straight lines in divers planes, does not make my enumeration insufficient. For those lines, though not parallels, nor cutting both the planes, yet being moved parallelly from one plane to another, will fall into one or other of the kinds of position by me enumerated; and consequently, are as much that position, as two straight lines in the same plane, not parallel, make the same angle, though not produced till they meet, which they would make if they were so produced: for you have nowhere proved, nor can prove, that two such lines do not make an angle. It is not the actual concurrence of the lines, but the arch of a circle, drawn upon that point for centre, in which

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they would meet if they were produced, and intercepted between them, that constitutes the angle.

Also your objection concerning asymptotes *in general* is absurd. You would have me add, that *their distance shall at last be less than any distance that can be assigned;* and so make the definition of the *genus* the same with that of the *species*. But because you are not professors of logic, it is not necessary for me to follow your counsel. In like manner, if we understand one line to be moved towards another always parallelly to itself, which is, though not actually, yet potentially the same position, all the rest of your instances will come to nothing.

At the two-and-twentieth article you object to me the use of the word *figure*, before I had defined it: wherein also you do absurdly; for I have nowhere before made such use of the word *figure*, as to argue anything from it; and therefore your objection is just as wise as if you had found fault with putting the word figure in the titles of the chapters placed before the book. If you had known the nature of demonstration, you had not objected this.

You add further, that by my definition of *figure*, a solid sphere, and a sphere made hollow within, is the same figure; but you say not why, nor can you derive any such thing from my definition. That which deceived your shallowness, is, that you take those points that are in the concave superficies of a hollowed sphere, not to be contiguous to anything without it, because that whole concave superficies is within the whole sphere. Lastly, for the fault you find with the definition of *like*

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figures in like positions, I confess there wants the same word which was wanting in the definition of parallels; namely, *ad easdem partes (the same way)* which should have been added in the end of the definition of like figures, &c., and may easily be supplied by any student of geometry, that is not otherwise a fool.

At the fifteenth chapter, art. 1, number 6, you object as a contradiction, that *I make motion to be the measure of time; and yet, in other places, do usually measure motion and the affections thereof by time.* If your thoughts were your own, and not taken rashly out of books, you could not but, (with all men else that see time measured by clocks, dials, hour-glasses, and the like), have conceived sufficiently, that there cannot be of time any other measure besides motion; and that the most universal measure of motion, is a line described by some other motion; which line being once exposed to sense, and the motion whereby it was described sufficiently explicated, will serve to measure all other motions and their time: for time and motion (time being but the mental image or remembrance of the motion) have but one and the same dimension, which is a line. But you, that would have me measure *swiftness* and *slowness* by longer and shorter motion, what do you mean by *longer* and *shorter motion?* Is *longer* and *shorter* in the motion, or in the duration of the motion, which is time? Or is the motion, or the duration of the motion, that which is exposed, or designed by a line? Geometricians say often, *let the line A B be the time;* but never say, *let the line A B be the motion.* There is no unlearned man that under-

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standeth not what is time, and motion, and measure; and yet you, that undertake to teach it (most egregious professors) understand it not.

At the second article you bring another argument (which it seems in its proper place you had forgotten), to prove that a point is not quantity not considered, but absolutely nothing; which is this, *That if a point be not nothing, then the whole is greater than its two halves.* How does that follow? Is it impossible when a line is divided into two halves, that the middle point should be divided into two halves also, being quantity?

At the seventh article, I have sufficiently demonstrated, that all motion is infinitely propagated, as far as space is filled with body. You allege no fault in the demonstration, but object from sense, *that the skipping of a flea is not propagated to the Indies.* If I ask you how you know it, you may wonder perhaps, but answer you cannot. Are you philosophers, or geometricians, or logicians more than are the simplest of rural people? or are you not rather less, by as much as he that standeth still in ignorance, is nearer to knowledge, than he that runneth from it by erroneous learning?

And, lastly, what an absurd objection is it which you make to the eighth article, where I say that *when two bodies of equal magnitude fall upon a third body, that which falls with greater velocity, imprints the greater motion?* You object, *that not so much the magnitude is to be considered as the weight;* as if the weight made no difference in the velocity, when notwithstanding weight is nothing else but motion downward. Tell me, when a weighty body thrown upwards worketh on the body it meeteth with, do you not then think it worketh the more for the greatness, and the less for the weight.

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OF THE FAULTS THAT OCCUR IN DEMONSTRATION.

TO THE SAME EGREGIOUS PROFESSORS OF THE MATHEMATICS IN THE UNIVERSITY OF OXFORD.

LESSON IV.

Of twenty articles which you say (of nineteen which I say) make the sixteenth chapter, you except but three, and confidently affirm the rest are false. On the contrary, except three or four faults, such as any geometrician may see proceed not from ignorance of the subject, or from want of the art of demonstration, (and such as any man might have mended of himself) but from security; I affirm that they are all true, and truly demonstrated; and that all your objections proceed from mere ignorance of the mathematics.

The first fault you find is this, that I express not (art. 1.) what *impetus* it is, which I would have to be multiplied into the time.

The last article of my thirteenth chapter was this, "*If there be a number of quantities propounded, howsoever equal or unequal to one another; and there be another quantity which so often taken as there be quantities propounded, is equal to their whole sum; that quantity I call the mean arithmetical of them all.*" Which definition I did there insert to serve me in the explication of those

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propositions of which the sixteenth chapter consisteth, but did not use it here as I intended. My first proposition therefore as it standeth yet in the Latin, being this, "*the velocity of any body moved during any time, is so much as is the product of the* impetus *in one point of time, multiplied into the whole time;*" to a man that hath not skill enough to supply what is wanting, is not intelligible. Therefore I have caused it in the English to go thus: "*the velocity of any body in whatsoever time moved, hath its quantity determined by the sum of all the several* (impetus) *quicknesses, which it hath in the several points of the time of the body's motion.* And added, *that all the* impetus *together taken through the whole time is the same thing with the mean* impetus (which mean is defined (Chapter XIII. art. 29) *multiplied into the whole time.*" To this first article, as it is uncorrected in the Latin, you object, *that meaning by* impetus *some middle* impetus, *and assigning none, I determine nothing*. And it is true. But if you had been geometricians sufficient to be professors, you would have shewed your skill much better, by making it appear that this middle *impetus* could be none but that, which being taken so often, as there be points in the line of time, would be equal to the sum of all the several *impetus* taken in the points of time respectively; which you could not do.

To the *corollary*, you ask first how *impetus* can be ordinately applied to a line; absurdly. For does not Archimedes sometimes say, and with him many other excellent geometricians, *let such a line be the time?* And do they not mean, that that

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line, or the motion over it, is the measure of the time? And may not also a line serve to measure the swiftness of a motion? *You thought*, you say, *only lines ought to be said to be ordinately applied to lines*. Which I easily believe; for I see you understand not that a line, though it be not the time itself, may be the quantity of a time. You thought also, all you have said in your *Elenchus*, in your doctrine of the *angle of contact*, in your *Arithmetica Infinitorum*, and in your *Conics*, is true; and yet it is almost all proved false, and the rest nothing worth.

Secondly, you object, that *I design a parallelogram by one only side*. It was indeed a great oversight, and argueth somewhat against the man, but nothing against his art. For he is not worthy to be thought a geometrician that cannot supply such a fault as that, and correct his book himself. Though you could not do it, yet another from beyond sea took notice of the same fault in this manner, " *He maketh a parallelogram of but one side;* it should be thus: *vel denique per parallelogrammum cujus unum latus est medium proportionale inter impetum maximum (sive ultimo acquisitum) et impetus ejusdem maximi semissem; alterum vero latus, medium proportionale, inter totum tempus, et ejusdem totius temporis semissem.*" Which I therefore repeat, that you may learn good manners; and know, that they who reprehend, ought also, when they can, to add to their reprehension the correction.

At the second article, you are pleased to advise me, instead of *in omni motu uniformi*, to put in *in omnibus motibus uniformibus*. You have a strange

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opinion of your own judgment, to think you know to what end another man useth any word, better than himself. My intention was only to consider motions uniform, and motions from rest uniformly, or regularly accelerated, that I might thereby compute the lengths of crooked lines, such as are described by any of those motions. And therefore it was enough to prove this theorem to be true in all uniform or uniformly accelerated *motion*, not *motions ;* though it be true also in the plural. It seems you think a man must write all he knows, whether it conduce, or not, to his intended purpose. But that you may know that I was not (as you think), ignorant how far it might be extended, you may read it demonstrated at the same article in the English universally. Against the demonstration itself you run into another article, namely, the thirteenth, which is this problem : " *the length being given, which is passed over in a given time by uniform motion, to find the length which shall be passed over by motion uniformly accelerated in the same time, so as that the* impetus *last acquired be equal to the time*." Which you recite imperfectly, thereby to make it seem that such a length is not determined. Whether you did this out of ignorance, or on purpose, thinking it a piece of wit, as your pretended mystery which goes immediately before, I cannot tell, for in neither place can any wit be espied by any but yourselves. To imagine motions with their times and ways, is a new business, and requires a steady brain, and a man that can constantly read in his own thoughts, without being diverted by the noise of words. The want of this ability, made you stumble and fall unhand-

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somely in the very first place (that is in Chap. XIII. art. 13), where you venture to reckon both motion and time at once; and hath made you in this chapter to stumble in the like manner at every step you go. As, for example, when I say, *as the product of the time, and impetus, to the product of the time and impetus, so the space to the space when the motion is uniform;* you come in with, *nay, rather as the time to the time;* as if the parallelograms A I, and A H, were not also as the times A B, and A F. Thus it is, when men venture upon ways they never had been in before, without a guide.

In the corollary, you are offended with the permutation of the proportion of times and lines, because you think, (you that have scarce one right thought of the principles of geometry), that line and time are heterogeneous quantities. I know time and line are of divers natures; and more, that neither of them is *quantity*. Yet they may be both of them *quanta*, that is, they may *have quantity;* but that their quantities are heterogeneous is false. For they are compared and measured both of them by straight lines. And to this there is nothing contrary in the place cited by you out of Clavius; or if there were, it were not to be valued. And to your question, what is the proportion of an *hour* to an *ell?* I answer, it is the same proportion that *two hours* have to *two ells*. You see your question is not so subtle as you thought it. By and bye you confess that in times and lines there is *quid homogeneum* (this *quid* is an infallible sign of not fully understanding what you say); which is false if you take it of the lines the faults t occur in nonstration.

themselves; though if you take it of their quantities, it is true without a *quid*. Lastly, you tell me how I might have expressed myself so as it might have been true. But because your expressions please me not, I have not followed your advice.

To the third article, which is this: "*In motu uniformiter a quiete accelerato,*" *etc.* "*In motion uniformly accelerated from rest, that is, when the impetus increaseth in proportion to the times, the length run over in one time is to the length run over in another time, as the product of the impetus multiplied by the time, to the product of the impetus multiplied by the time;*" you object, "*that the lengths run over are in that proportion which the impetus hath to the impetus; not that which the impetus hath to the time, because impetus to time has no proportion, as being heterogeneous.*" First, when you say the impetus, do you mean some one impetus designed by some one of the unequal straight lines parallel to the base B I? That is manifestly false. You mean the aggregate of all those unequal parallels. But that is the same thing with the time multiplied into the mean impetus. And so you say the same that I do. Again, I ask, where it is that I say or dream that the lengths run over are in the proportion of the impetus to the times? Is it you or I that dream? And for your heterogeneity of the quantities of time and of swiftness, I have already in divers places showed you your error. Again, why do you make B I represent the lengths run over, which I make to be represented by D E, a line taken at pleasure; and you also a few lines before make the same

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B I to design the greatest acquired impetus? These are things which show that you are puzzled and entangled with the unusual speculation of time and motion, and yet are thrust on with pride and spite to adventure upon the examination of this chapter.

Secondly, you grant the demonstration to be good, supposing I mean it, as I seem to speak, of one and the same motion. But why do I not mean it of one and the same motion, when I say not in *motions*, but in *motion* uniform? *Because,* say you, *in that which follows, I draw it also to different motions.* You should have given at least one instance of it; but there is no such matter. And yet the proposition is in that case also true; though then it must not be demonstrated by the similitude of triangles, as in the case present. And therefore the objections you make from different impetus acquired in the same time, and from other cases which you mention, are nothing worth.

At the fourth article, you allow the demonstration all the way (except the faults of the third, which I have already proved to be none) till I come to say, "*that because the proportion of F K to B I is double to the proportion of A F to A B, therefore the proportion of A B to A F is double to the proportion of B I to F K.*" This you deny, and wonder at as strange, (for it is indeed strange to you), and in many places you exclaim against it as extreme ignorance in geometry. In this place you only say, "*no such matter; for though one proportion be double to another, yet it does not follow that the converse is the double of the converse.*" So that this is the issue to which the

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question is reduced, whether you have any or no geometry. I say, if there be three quantities in continual proportion, and the first be the least, the proportion of the first to the second is double to the proportion of the first to the third; and you deny it. The reason of our dissent consisteth in this, that you think the doubling of a proportion to be the doubling of the quantity of the proportion, as well in proportions of defect, as in proportions of excess; and I think that the doubling of a proportion of defect, is the doubling of the defect of the quantity of the same. As for example in these three numbers, 1, 2, 4, which are in continual proportion, I say the quantity of the proportion of one to two, is double the quantity of the proportion of one to four. And the quantity of the proportion of one to four, is half the quantity of the proportion of one to two. And yet deny not but that the quantity of the defect in the proportion of one to two is doubled in the proportion of one to four. But because the doubling of defect makes greater defect, it maketh the quantity of the proportion less. And as for the part which I hold in this question, first, there is thus much demonstrated by Euclid, El. v. prop. 8; that the proportion of one to two, is greater than the proportion of one to four, though how much it is greater be not there demonstrated. Secondly, I have demonstrated (Chap. XIII. art. 16); that it is twice as great, that is to say, (to a man that speaks English), double. The introducing of *duplicate*, *triplicate*, &c. instead of *double*, *triple*, &c. (though now they be words well understood by such as understand what proportion is), proceeded at first from such as durst not for

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fear of absurdity, call the half of any thing double to the whole, though it be manifest that the half of any defect is a double quantity to the whole defect; for want added to want maketh greater want, that is, a less positive quantity. This difference between *double* and *duplicate*, lighting upon weak understandings, has put men out of the way of true reasoning in very many questions of geometry. Euclid never used but one word both for *double* and *duplicate*. It is the same fault when men call half a quantity *subduplicate*, and a third part *subtriplicate* of the whole, with intention (as in this case) to make them pass for words of signification different from the *half* and the *third part*. Besides, from my definition of proportion (which is clear, and easy to be understood by all men, but such as have read the geometry of others unluckily) I can demonstrate the same evidently and briefly thus. My definition is this, *proportion is the quantity of one magnitude taken comparatively to another*. Let there be therefore three quantities, 1, 2, 4, in continual proportion. Seeing therefore the quantity of four in respect of one, is twice as great as the quantity of the same four in respect of 2, it followeth manifestly that the quantity of 1 in respect of 4, is twice as little as the quantity of the same 1 in respect of 2; and consequently the quantity of 1 in respect of 2, is twice as great as the quantity of the same 1 in respect of 4; which is the thing I maintain in this question. Would not a man that employs his time at bowls, choose rather to have the advantage given him of three in nine, then of one in nine? And why, but that three is a greater quantity in respect of nine, than

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is one? Which is as much as to say, three to nine hath a greater proportion than one to nine; as is demonstrated by Euclid, El. v. prop. 8. Is it not therefore (you that profess mathematics, and theology, and practise the depression of the truth in both) well owled of you, to teach the contrary? But where you say "*that the point K* (in the second figure of the table belonging to this sixteenth chapter) *is not in the parabolical line whose diameter is A B, and base B I, but in the parabolical line of the complement of my semiparabola (as I may learn from the twenty-third proposition of your* Arithmetica Infinitorum) *whose diameter is A C, and base I C.*" What line is that? Is it the same line with that of my semiparabola, or not the same? If the same, why find you fault? If not the same, you ought to have made a semiparabola on the diameter A C, and base I C, and following my construction made it appear that K is not in the line wherein I say it is; which you have not done, nor could do.

Then again, running on in the same blindness of passion, you pretend I make the proportion of B I to F K double to that of A B to A F, and then confute it; when you knew I made the proportion of F K to B I, double to that of F N, to B I, that is, of A F to A B; and this was it you should have confuted. That which followeth is but a triumphing in your own ignorance, wherein you also say, "*that all that I afterwards build upon this doctrine is false.*" You see whether it be like to prove so or not. As for your *Arithmetica Infinitorum*, I shall then read to you a piece of a lesson on it when I come to your objections against the next

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Chapter. In the mean time let me tell you, it is not likely you should be great geometricians, that know not what is quantity, nor measure, nor straight, nor angle, nor homogeneous, nor heterogeneous, nor proportion, as I have already made appear in this and the former lessons.

To the first corollary of this fourth article your exception I confess is just, and (which I wonder at) without any incivility. But this argues not ignorance, but security. For who is there that ever read any thing in the Conics, that knows not that the parts of a parabola cut off by lines parallel to the base, are in triplicate proportion to their bases? But having hitherto designed the time by the diameter, and the impetus by the base; and in the next chapter (where I was to calculate the proportion of the parabola, to the parallelogram) intending to design the time by the base, I mistook and put the diameter again for the time; which any man but you might as easily have corrected as reprehended.

To the second corollary, which is this, *that the lengths run over in equal times by motion so accelerated, as that the impetus increase in double proportion to their times, are as the differences of the cubic numbers beginning at unity, that is, as seven, nineteen, thirty-seven, &c.* you say it is false. But why? "*Because*" say you "*portions of the parabola of equal altitude, taken from the beginning, are not as those numbers seven, nineteen, thirty-seven, &c.*" Does this, think you, contradict any thing in this proposition of mine? Yes, because, you think, the lengths gone over in equal times, are the same with the parts of the

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diameter cut off from the vertex, and proportional to the numbers one, two, three, &c. Whereas the lengths run over, are as the aggregates of their velocities, that is, as the parts of the parabola itself, that is, as the cubes of their bases, that is, as the numbers one, eight, twenty-seven, sixty-four, &c., and consequently the lengths run over in equal times, are as the differences of those cubic numbers, one, eight, twenty-seven, sixty-four, whose differences are seven, nineteen, thirty-seven, &c. The cause of your mistake was, that you cannot yet, nor perhaps ever will, contemplate time and motion (which requireth a steady brain) without confusion.

The third corollary you also say is false, "*whether it be meant of motion uniformly accelerated* (as the words are) *or (as perhaps*, you say, *I meant it) of such motion as is accelerated in double proportion to the time.*" You need not say perhaps I meant it. The words of the proposition are enough to make the meaning of the corollary understood. But so also you say it is false. Methinks you should have offered some little proof to make it seem so. You think your authority will carry it. But on the contrary I believe rather that they that shall see how your other objections hitherto have sped, will the rather think it true, because you think it false. The demonstration as it is, is evident enough; and therefore I saw no
change a word of it.

rticle you object nothing, but that
his proposition (Chap. XIII. art.
en three quantities are in con-
and the first is the least, as in

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these numbers, four, six, nine, the proportion of the first to the second, is double to the proportion of the same first to the last;" which is there demonstrated, and in the former lessons so amply explicated, as no man can make any further doubt of the truth of it. And you will, I doubt not, assent unto it. But in what estate of mind will you be then? A man of a tender forehead after so much insolence, and so much contumelious language grounded upon arrogance and ignorance, would hardly endure to outlive it. In this vanity of yours, you ask me whether I be angry, or blush, or can endure to hear you. I have some reason to be angry; for what man can be so patient as not to be moved with so many injuries? And I have some reason to blush, considering the opinion men will have beyond sea, (when they shall see this in Latin) of the geometry taught in Oxford. But to read the worst you can say against me, I can endure, as easily at least, as to read any thing you have written in your treatises of the *Angle of Contact,* of the *Conic Sections,* or your *Arithmetica Infinitorum.*

The sixth, seventh, eighth articles, you say are sound. True. But never the more to be thought so for your approbation, but the less; because you are not fit, neither to reprehend, nor praise; and because all that you have hitherto condemned as false, hath been proved true. Then you show me how you could demonstrate the sixth and seventh articles a shorter way. But though there be your symbols, yet no man is obliged to take them for demonstration. And though they be granted to be dumb demonstrations, yet when they are taught to speak as they ought to do, they will be longer demonstrations than these of mine.

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To the ninth article, which is this, "*If a body be moved by two movents at once, concurring in what angle soever, of which, one is moved uniformly, the other, with motion uniformly accelerated from rest, till it acquire an impetus equal to that of the uniform motion, the line in which the body is carried, shall be the crooked line of a semiparabola,*" you lift up your voice again, and ask, *what latitude? what diameter? what inclination* of the diameter to the ordinate lines? If your founder should see this, or the like objections of yours, he would think his money ill bestowed. When I say, *in what angle soever*, you ask, *in what angle?* When I say *two movents, one uniform, the other uniformly accelerated, make the body describe a semiparabolical line;* you ask, *which is the diameter?* as not knowing that the accelerated motion describes the diameter, and the other a parallel to the base. And when I say *the two movents meet in a point, from which point both the motions begin, and one of them from rest,* you ask me *what is the altitude?* As if that point where the motion begins from rest were not the vertex; or that the vertex and base being given, you had not wit enough to see that the altitude of the parabola is determined? When Galileo's proposition, which is the same with this of mine, supposed no
but a body moved by these two motions, to
described to be the crooked line of
never thought of asking him
what diameter, nor what angle,
l his parabola. And when

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Archimedes said, let the line A B be the time, I should never have said to him, *do you think time to be a line*, as you ask me whether I think impetus can be the base of a parabola. And why, but because I am not so egregious a mathematician, as you are. In this giddiness of yours, caused by looking upon this intricate business of motion, and of time, and the concourse of motion uniform, and uniformly accelerated, you rave upon the numbers 1, 4, 9, 16, &c. without reference to any thing that I had said; insomuch as any one that had seen how much you have been deceived in them before, in your scurvy book of *Arithmetica Infinitorum*, would presently conclude, that this objection was nothing else but a fit of the same madness which possessed you there.

My tenth article is like my ninth; and your objections to it are the same which are to the former. Therefore you must take for answer just the same which I have given to your objection there.

To the eleventh, you say first, you have done it better at the sixty-fourth article of your *Arithmetica Infinitorum*. But what you have done there, shall be examined when I come to the defence of my next chapter. And whereas I direct the reader for the finding of the proportions of the complements of those figures to the figures themselves, to the table of art. 3, Chap. XVII., you say that if the increase of the *spaces*, were to the increase of the times, as one to two, then the complement should be to the parallelogram as one to three, and say you find not $\frac{1}{3}$ in the table. Did you not see that the table is only of those figures which are

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described by the concourse of a motion uniform with a motion accelerated? You had no reason therefore to look for $\frac{1}{3}$ in that table; for your case is of motion uniform concurring with motion retarded, because you make not the proportions of the spaces to the proportions of the times as two to one, but the contrary; so that your objection ariseth from want of observing what you read. But I "*may learn*" you say, "*these, and greater matters than these, in your twenty-third and sixty-fourth propositions of your* Arithmetica Infinitorum." This, which you say here is a great absurdity; but if you mean I shall find greater there, I will not say against you. This $\frac{1}{3}$ you looked for, belongs to the complements of the figures calculated in that table; which because you are not able to find out of yourselves, I will direct you to them. Your case is of $\frac{1}{3}$ for the complement of a parabola. Take the denominator of the fraction which belongs to the parabola, namely three, and for numerator take the numerator of the fraction which belongs to the triangle, namely one, and you have the fraction sought. And in like manner for the complement of any other figure. As, for example, of the second parabolaster, whose fraction hath for denominator five, take the numerator of the fraction of the same triangle which is one, and you have $\frac{1}{5}$ for the fraction sought for; and so of the rest, taking always one for the numerator.

The twelfth article, which you say is miserably false, I have left standing unaltered. For not comprehending the sense of the proposition, you make a figure of your own, and fight against your

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own fancied motions, different from mine. Other geometricians that understand the construction better, find no fault. And if you had in your own fifth figure drawn a line through N parallel to A E, and upon that line supposed your accelerated motion, you would quickly have seen that in the time A E, the body moved from rest in A, would have fallen short of the diagonal A D; and that all your extravagant pursuing of your own mistake had been absurd.

My thirteenth article you say is ridiculous. But why? "*The impetus last acquired cannot*" you say, "*be equal to a time.*" But the quantity of the impetus may be equal to the quantity of a time, seeing they are both measured by line. And when they are measured by the same described line, each of their quantities is equal to that same line, and consequently to one another. But when I meet with this kind of objection again, since I have so often already shown it to be frivolous, and no less to be objected against all the ancients that ever demonstrated any thing by motion, than against me, I purpose to neglect it.

Secondly, you object "*that motion uniformly accelerated does no more determine swiftness, than motion uniform.*" True; you needed not have used sixteen lines to set down that. But suppose I add, as I do, so as the last acquired impetus be equal to the time. *But that*, you say, *is not sense;* which is the objection I am to neglect. But, you say again, supposing it sense, this limitation helps me nothing. Why? *Because*, you say, *a parabola may be described upon a base given, and yet have any altitude, or any diameter*

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one will. Who doubts it? But how follows it from thence, that when a parabolical line is described by two motions, one uniform, the other uniformly accelerated from rest, that the determining of the base does not also determine the whole parabola? But fifthly, you say, *that this equality of the impetus to the time does not determine the base.* Why not? *Because,* you say, *it is an error proceeding from this, that I understand not what is* ratio subduplicata. I looked for this. I have shown and inculcated sufficiently before, but the error is on your side; and therefore must tell you, that this objection, and also a great part of the rest of your errors in geometry, proceedeth from this, that you know not what proportion is. But see how wisely you argue about this duplication of proportion. For thus you say *verbatim.* "*Stay a little. What proportion has duplicate proportion to single proportion? Is it always the same? I think not for example, duplicate proportion* $\frac{4}{1}=\frac{2}{1}\text{ in }\frac{2}{1}$ *is double to the single* $\frac{2}{1}$. *Duplicate proportion* $\frac{9}{1}=\frac{3}{1}\text{ in }\frac{3}{1}$ *is triple to its single* $\frac{3}{1}$." Let any man, even of them that are most ready in your symbols, say in your behalf (if he be not ashamed) that the proportion of nine to one is triple to the proportion of three to one, as you do.

In the fourteenth, fifteenth, and sixteenth articles, you bid me repeat your objections to the thirteenth. I have done it; and find that what you have objected to the thirteenth, may as well be objected to these; and consequently, that my answer there will also serve me here. Therefore, if you can endure it, read the same answer over again.

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But you have not yet done, you say, with these articles. Therefore (after you had for a while spoken perplextly, conjecturing, not without just cause, that I could not understand you) you say that to the end I may the better perceive your meaning, I should take the example following. "*Let a movent (in the first figure of this chapter) be moved uniformly in the time A B, with the continual impetus A C, or B I, whose whole velocity shall therefore be the parallelogram A C I B. And another movent be uniformly accelerated, so as in the time A B it acquire the same impetus B I. Now as the whole velocity, is to the whole velocity, so is the length run over, to the length run over.*" All this I acknowledge to be according to my sense, saving that your putting your word *movens* instead of my word *mobile* hath corrupted this article. For in the first article, I meddle not with motion by concourse, wherein only I have to do with two movents to make one motion; but in this I do, wherein my word is not *movens* but *mobile*; by which it is easy to perceive you understand not this proposition. Then you proceed: "*But the length run over by that accelerated motion is greater than the length run over by that uniform motion.*" Where do I say that? You answer, "*in the ninth and thirteenth article, in making A B (in the fifth figure) greater than A C; and A H (in the eighth figure) greater than A B: and consequently, the triangle A B I, greater than the parallelogram A C I B.*" That consequently is without consequence; for it importeth nothing at all in this demonstration, whether A B, or A C in the fifth figure be the greater.

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Besides I speak there of the concourse of two movents, that describe the parabolical line A G D where the increasing impetus (because it increaseth as the times) will be designed by the ordinate lines in the parabola A G D B. And if both the motions in A B and A C were uniform, the aggregate of the impetus would be designed by the triangle A B D, which is less than the parallelogram A C D B. But you thought that the motion made by A C uniformly, is the same with the motion made uniformly in the same time by the motions in A B and A C concurring; so likewise, in the eighth figure, there is nothing hinders A H from being greater than A B, unless I had said that A B had been described in the time A C with the whole impetus A C maintained entire; of which there is nothing in the proposition, nor would at all have been pertinent to it. Therefore all this new undertaking of the thirteenth, fourteenth, fifteenth, and sixteenth articles, is to as little purpose as your former objections. But I perceive that these new and hard speculations, though they turn the edge of your wit, turn not the edge of your malice.

At the seventeenth article, you show again the same confusion. Return to the eighth figure: "*if in a time given a body run over two lengths, one with uniform, the other with accelerated motion*"; as for example, if in the same time A C, a body, run over the line A B with uniform motion, and the line A H with motion accelerated; "*and again in a part of that time it run over a part of the length A H, with uniform motion, and another part of the same with motion accelerated;*" as for

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example, in the time A M it run over with uniform motion the line A I, and with motion accelerated the line A B. *I say the excess of the whole A H above the part A B, is to the excess of the whole A B above the part A I, as the whole A H to the whole A B.* But first you will say, that these words *as the whole A H to the whole A B,* are left out in the proposition. But you acknowledge that it was my meaning; and you see it is expressed before I come to the demonstration. And therefore it was absurdly done to reprehend it. Let us therefore pass to the demonstration. Draw I K parallel to A C, and make up the parallelogram A I K M. And supposing first the acceleration to be uniform, divide I K in the midst at N; and between I N, and I K, take a mean proportional I L. *And the straight line A L, drawn and produced, shall cut the line B D in F, and the line C G in G* (which lines C G, and B D, as also H G and B F, are determined, though you could not carry it so long in memory, by the demonstration of the thirteenth article). *For seeing A B is described by motion uniformly accelerated, and A I by motion uniform in the same time A M; and I L is a mean proportional between I N (the half of I K) and I K; therefore by the demonstration of the thirteenth article, A I is a mean proportional between A B and the half of A B, namely A O. Again, because A B is described by uniform motion, and A H by motion uniformly accelerated, both of them in the same time A C, B F is a mean proportional between B D and half B D, namely B E; therefore by the demonstration of the same thirteenth article, the straight line A L F pro-*

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duced will fall on G; and the line A H will be to the line A B, as the line A B to the line A I. And consequently as A H to A B, so H B to B I; which was to be demonstrated. And by the like demonstration the same may be proved, where the acceleration is in any other proportion that can be assigned in numbers, saving that whereas this demonstration dependeth on the construction of the thirteenth article, if the motion had been accelerated in double proportion to the times, it would have depended on the fourteenth, where the lines are determined. Which determinations being not repeated, but declared before, in the thirteenth article, to which this diagram belongeth, you take no notice of, but go back to a figure belonging to another article, where there was no use of these determinations. But because I see that the words of the proposition, are as of four motions, and not of two motions made by twice two movents, I must pardon them that have not rightly understood my meaning; and I have now made the proposition according to the demonstration. Which being done, all that you have said in very near two leaves of your *Elenchus* comes to nothing; and the fault you find comes to no more than a too much trusting to the skill and diligence of the reader. And whereas after you had sufficiently troubled yourself upon this occasion, you add, "*that if Sir H. Savile had read my Geometry, he had never given that censure of Joseph Scaliger, in his lecture upon Euclid, that he was the worst geometrician of all mortal men, not exceptioning so much as Orontius, but that praise should have been kept for me.*" You see by this time, at least

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hers do, how little I ought to value that opinion;
d that though I be the least of geometricians,
t my geometry is to yours as 1 to 0. I recite
ese words of yours, to let the world see your
liscretion in mentioning so needlessly that pas-
ge of your founder. It is well known that
seph Scaliger deserved as well of the state of
rning, as any man before or since him; and
it though he failed in his ratiocination concern-
g the quadrature of the circle, yet there appears
that very failing so much knowledge of geome-
, that Sir H. Savile could not but see that there
re mortal men very many that had less; and
nsequently he knew that that censure of his in a
gid sense (without the license of an hyperbole)
as unjust. But who is there that will approve
such hyperboles to the dishonour of any but of
worthy persons, or think Joseph Scaliger un-
orthy of honour from learned men? Besides, it
as not Sir H. Savile that confuted that false
adrature, but Clavius. What honour was it then
r him to triumph in the victory of another?
hen a beast is slain by a lion, is it not easy for
y of the fowls of the air to settle upon, and peck
m? Lastly, though it were a great error in
caliger, yet it was not so great a fault as the least
n; and I believe that a public contumely done to
y worthy person after his death, is not the least
sins. Judge therefore whether you have not
ne indiscreetly, in reviving the only fault, per-
ps that any man living can lay to your founder's
arge; and yet this error of Scaliger's was no
reater than one of your own of the like nature,
making the true spiral of Archimedes equal to

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half the circumference of the circle of the first revolution; and then thinking to cover your fault by calling it afterwards an aggregate of arches of circles (which is no spiral at all of any kind) you do not repair but double the absurdity. What would Sir Henry Savile have said to this?

The eighteenth article is this, "*in any parallelogram, if the two sides that contain the angle be moved to their opposite sides, the one uniformly, the other uniformly accelerated; the side that is moved uniformly, by its concourse through all its longitude, hath the same effect which it would have if the other motion were also uniform, and the line described were a mean proportional between the whole length, and the half of the same.*"

To the proposition you object first, "*that it is all one whether the other motion be uniform or not, because the effect of each of their motions, is but to carry the body to the opposite side.*" But do you think that whatsoever be the motions, the body shall be carried by their concourse always to the same point of the opposite side? If not, then the effect is not all one when a motion is made by the concourse of two motions uniform and accelerated, and when it is made by the concourse of two uniform or of two accelerated motions.

Secondly, you say that these words, *and the line described were a mean proportional between the whole length, and the half of the same*, have no sense, or that you are deceived. True. For you are deceived; or rather you have not understanding enough distinctly to conceive variety of motions though distinctly expressed. For when a line is gone over with motion uniformly accelera-

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ted, you cannot understand how a mean proportional can be taken between it and its half; or if you can, you cannot conceive that that mean can be gone over with uniform motion in the same time that the whole line was run over by motion uniformly accelerated. Yet these are things conceivable, and your want of understanding must be made my fault.

My demonstration is this, *in the parallelogram A B C D, (Fig.* 11*). Let the side A B be conceived to be moved uniformly till it lie in C D; and let the time of that motion be A C, or B D. And in the same time let it be conceived that A C is moved with uniform acceleration, till it lie in B D.* To which you object, *that then the acceleration last acquired must be far greater than that wherewith A B is moved uniformly: else it shall never come to the place you would have it in the same time.* What proof bring you for this? None here. Where then? Nowhere that I remember. On the contrary I have proved (Art. 9 of the chapter) that the line described by the concourse of those two motions, namely, uniform from A B to C D, and uniformly accelerated from A C to B D, is the crooked line of the semiparabola A H D. And though I had not, yet it is well known that the same is demonstrated by Galileo. And seeing it is manifest that in what proportion the motion is accelerated in the line A B, in the same proportion the impetus beginning from rest in A is increased in the same times (which impetus is designed all the way by the ordinate lines of the semiparabola), the greatest impetus acquired must needs be the base of the semiparabola, namely B D,

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equal to A C, which designs the whole time. I cannot therefore imagine what should make you say without proof, that the greatest acquired impetus is greater than that which is designed by the base B D. Next you say, " you see not to what end I divide A B in the middle at E." No wonder; for you have seen nothing all the way. Others would see it is necessary for the demonstration ; as also that the point F is not to be taken arbitrarily; and likewise that the thirteenth article, which you admit not for proof, is sufficiently demonstrated, and your objections to it answered. By the way you advise me, where I say ***percursam eodem motu uniformi, cum impetu ubique,*** &c. to blot out ***cum ;*** because the ***impetus*** is not a ***companion*** in the way, but the ***cause.*** Pardon me in that I cannot take your learned counsel ; for the word ***motu uniformi*** is the ablative of the ***cause***, and ***impetu*** the ablative of the ***manner***. But to come again to your objections, you say, I make " ***a greater space run over in the same time by the slower motion than by the swifter***." How does that appear ? ***because there is no doubt, but the swiftness is greater where the greatest impetus is always maintained, than where it is attained to in the same time from rest.*** True, but that is, when they are considered asunder without concourse, but not then when by the concourse they debilitate one another, and describe a third line different from both the lines, which they would describe singly. In this place compare their effects as contributing to the de- ription of the parabolical line A H D. What the cts of their several motions are, when they are isidered asunder, is sufficiently shown before in

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the first article. You should first have gotten into your minds the perfect and distinct ideas of all the motions mentioned in this chapter, and then have ventured upon the censure of them, but not before. And then you would have seen that the body moved from A, describeth not the line A C, nor the line A B, but a third, namely the semiparabolical line A H D.

Again, where I say, *Wherefore, if the whole A B be uniformly moved to C D, in the same time wherein A C is moved uniformly to F G ;* you ask me "*whether with the same impetus or not?*" How is it possible that in the same time two unequal lengths should be passed over the same impetus? "*But why,*" say you, "*do you not tell us with what impetus A C comes to F G?*" What need is there of that, when all men know that in uniform motion and the same time, impetus is to impetus, as length to length? Which to have expressed had not been pertinent to the demonstration. That which follows in the demonstration, *rursus suppono quod latus A C*, &c. to these words, *ut ostensum est*, *Art.* 12, you confute with saying you have proved that article to be false. But you may see now, if you please, at the same place that I have proved your objection to be frivolous.

After this you run on without any argument against the rest of the demonstration, showing nothing all the way, but that the variety and concourse of motions, the speculations whereof you have not been used to, have made you giddy.

To the nineteenth article you apply the same objection which you made to the eighteenth.

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Which having been answered, it appears that from the very beginning of your Elenchus to this place all your objections (except such as are made to three or four mistakes of small importance in setting down my mind), are mere paralogisms, and such are less pardonable than any paralogism in Orontius, both because the subject as less difficult is more easily mastered, and because the same faults are most shamefully committed by a reprehender than by any other man.

I had once added to these nineteen articles a twentieth, which was this: "*If from a point in the circumference there be drawn a cord, and a tangent equal to it, the angle which they make shall be double to the aggregate of all the angles made by the cords of all the equal arches into which the arch given can possibly be divided.*" Which proposition is true, and I did when I writ it think I might have use of it. But be it, or the demonstration of it true or false, seeing it was not published by me, it is somewhat barbarous to charge me with the faults thereof. No doctor of humanity but would have thought it a poor and wretched malice, publicly to examine and censure papers of geometry never published, by what means soever they came into his hands. I must confess that in these words, *in such kind of progression arithmetical* (that is, which begins with 0) *the sum of all the numbers taken together, is equal to half the number that is made by multiplying the greatest into the least,* there is a great error; for by this account these numbers, 0, 1, 2, 3, 4, taken together, should be equal to nothing. I should have said they are equal to that number which is made

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by multiplying half the greatest into the number of the terms. There was therefore, if those words were mine (for truly I have no copy of them, nor have had since the book was printed, and I have no great reason, as any man may see, to trust your faith) a great error in the writing, but not an erroneous opinion in the writer. The demonstration so corrected is true. And the angles that have the proportions of the numbers 1, 2, 3, 4, are in the table of your *Elenchus*, fig. 12, the angles GAD, HDE, IEF, KFB. And if the divisions were infinite, so that the first were not to be reckoned but as a cypher, the angle CAB would be double to them altogether. This mistake of mine, and the finding that I had made no use of it in the whole book, was the cause why I thought fit to leave it quite out. But your professorships, could not forbear to take occasion thereby, to commend your zeal against *Leviathan* to your doctorships of divinity, by censuring it.

OF THE FAULTS THAT OCCUR IN DEMONSTRATION.

TO THE SAME EGREGIOUS PROFESSORS OF THE MATHEMATICS IN THE UNIVERSITY OF OXFORD.

LESSON V.

At the seventeeth chapter, your first exception is to the definition of proportional proportions, which is this: " *Four proportions are then proportional, when the first is to the second, as the third to the*

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fourth." The reader will hardly believe that your exception is in earnest. You say, I mean not by proportionality the "*quantity of the proportions.*" Yes I do. Therefore I say again, that *four proportions are then proportional, when the quantity of the first proportion, is to the quantity of the second proportion, as the quantity of the third proportion, to the quantity of the fourth proportion.* Is not my meaning now plainly enough expressed? Or is it not the same definition with the former. But what do I mean, you will say, by the quantity of a proportion? I mean the determined greatness of it, that is, for example, in these numbers, the quantity of the proportion of two to three, is the same with the quantity of the proportion of four to six, or six to nine; and again, the quantity of the proportion of six to four, is the same with the quantity of the proportion of nine to six, or of three to two. But now what do you mean by the quantity of a proportion? You mean that two and three, are the quantities of the proportion of two to three (for so Euclid calls them) and that six and four are the quantities of the proportion of six to four, which is the same with the proportion of three to two. And by this rule, one and the same proportion shall have an infinite number of quantities; and consequently the quantity of a proportion can never be determined. I call one proportion double to another, when one is equal to twice the other; as the proportion of four to one, is double to the proportion of two to one. You call that proportion double where one number, line, or quantity absolute, is double to the other; so that with you the propor-

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tion of two to one is a double proportion. It is easy to understand how the number two is double to one, but to what, I pray you, is double the proportion of two to one, or of one to two? Is not every double proportion double to some proportion? See whether this geometry of yours can be taken by any man of sound mind for sense. "*But it is known*," you say, "*that in proportions, double is one thing, and duplicate another;*" so that it seems to you, that in talking of proportion men are allowed to speak senselessly. "*It is known,*" you say. To whom? It is indeed in use at this day to call *double duplicate*, and *triple triplicate*. And it is well enough; for they are words that signify the same thing, but that they differ (in what subject soever) I never heard till now. I am sure that Euclid, whom you have undertaken to expound, maketh no such difference. And even there where he putteth these numbers, one, two, four, eight, &c. for numbers in *double* proportion (which is the last proposition of the ninth element) he meaneth not that one to two, or two to one, is a *double* proportion, but that every number in that progression is *double* to the number next before it; and yet he does not call it *analogia dupla*, but *duplicate*. This distinction in proportions between *double* and *duplicate*, proceeded long after from want of knowledge that the proportion of one to two is *double* to the proportion of one to four; and this from ignorance of the different nature of proportions of *excess*, and proportions of *defect*. And you that have nothing but by tradition saw not the absurdities that did hang thereon.

In the second article I make EK, (fig. 1) the

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third part of L K, which you say is false; an consequently the proposition undemonstrated And thus you prove it false: "*Let A C be to G C or G K to G L, as eight to one (for seeing th point G is taken arbitrarily, we may place i where we will, &c.)*" and upon this placing of (arbitrarily, you prove well enough that E K is no a third part of L K. But you did not then observe, that I make *the altitude A G, less than an quantity given,* and by consequence E K to diffe from a third part by a less difference than an quantity that can be given. Therefore as yet th demonstration proceedeth well enough. But perceiving your oversight, you thought fit (thoug before, you thought this confutation sufficient) t endeavour to confute it another way; but wit much more evidence of ignorance. For when come to say, *the proportion therefore between A C and G C is triple, in arithmetical proportion, t the proportion between G K and G E, &c.* yo say, "*the proportion of A C to G C is the proportion of identity, as also that of G K to G E,*" But why? Does my construction make it so? D not I make G C less than A C, though with less difference than any quantity that can be assigned And then where I say, *therefore E K is the thir part of L K,* you come in, by parenthesis, wit *(or a fourth, or a fifth, &c.)* Upon what ground Because you think it will pass for current, withou proof, that a point is nothing. Which if it do, geometry also shall pass for nothing, as having no ground nor beginning but in nothing. But I have already in a former lesson sufficiently showed you the consequence of that opinion. To which I may

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add, that it destroys the method of *indivisibles,* invented by Bonaventura; and upon which, not well understood, you have grounded all your scurvy book of *Arithmetica Infinitorum;* where your indivisibles have nothing to do, but as they are supposed to have quantity, that is to say, to be *divisibles.* You allow, it seems, your own nothings to be somethings, and yet will not allow my somethings to be considered as nothing. The rest of your objections having no other ground than this, "*that a point is nothing,*" my whole demonstration standeth firm; and so do the demonstrations of all such geometricians, ancient and modern, as have inferred any thing in the manner following, viz. *If it be not greater nor less, then it is equal. But it is neither greater nor less. Therefore, &c. If it be greater, say by how much. By so much. It is not greater by so much. Therefore it is not greater. If it be less, say how much, &c.* Which being good demonstrations are together with mine overthrown by the nothingness of your *point,* or rather of your understanding; upon which you nevertheless have the vanity of advising me what to do, if I demonstrate the same again; meaning I should come to your false, impossible, and absurd method of *Arithmetica Infinitorum,* worthy to be gilded, I do not mean with gold.

And for your question, why I set the base of my figure upwards, you may be sure it was not because I was afraid to say, that the proportions of the ordinate lines beginning at the vertex were triplicate, or otherwise multiplicate of the proportions of the intercepted parts of the diameter. For I never doubted to call double duplicate, nor

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triple triplicate, &c., or if I had, I should have avoided it afterwards at the tenth article of the same chapter. But because when I went about to compare the proportions of the ordinate lines with those of their contiguous diameters, the first thing I considered in them was in what manner the base grew less and less till it vanished into a point. And though the base had been placed below, it had not therefore required any change in the demonstration. But I was the more apt to place the base uppermost, because the motion began at the base, and ended at the vertex. To proceed which way I pleased was in my own choice; and it is of grace that I give you any account of it at all.

To the third article, together with its table, you say, "*it falls in the ruin of the second; and that the same is to be understood of the sixth, seventh, eighth, and ninth.*" For confutation whereof I need to say no more, but that they all stand good by the confutation of your objections to the second.

To the fourth article you say, "*the description of those curvilineal figures is easy.*" True, to some men; and now that I have showed you the way, it is easy enough for you also. For the way you propound is wholly transcribed out of the figure of the second article, which article you had before rejected. For seeing the lines H F, G E, A B, &c. are equal to the lines C Q, C O, C D; and the lines Q F, O E, B D, equal to the lines C H, C G, C A; the proportion of D B to O E, will be triple (that is, triplicate) to the proportion of C O to G E; and the proportion of D B to Q F, triple to the proportion of C D to C Q; and consequently,

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because the complement B D C F E B is made by the decrease of A C in triple proportion to that of the decrease of C D, it will be (by the second article) a third part of the figure A B E F C A. So that it comes all to one pass, whether we take triple proportion in decreasing to make the complement, or triple proportion in increasing to make the figure; for the proportion of H F to B A, is triple to the proportion of C H to C A. Wherefore you have done no more but what you have seen first done, saving that from your construction you prove not the figure to be triple to the complement; perhaps because you have proved the contrary in your *Arithmetica Infinitorum.* But your way differs from mine, in that you call the proportion subtriplicate, which I call triplicate; as if the divers naming of the same thing made it differ from itself. You might as well have said briefly, the proposition is true, but ill proved, because I call the proportion of one or two triple, or triplicate of that of one to eight; which you say is false, and hath infected the fourth, fifth, ninth, tenth, eleventh, thirteenth, fourteenth, fifteenth, sixteenth, seventeenth, and nineteenth articles of the sixteenth chapter. But I say, and you know now, that it is true; and that all those articles are demonstrated.

Lastly you add, "*Tu vero, in presente articulo, &c. id est, you bid find as many mean proportionals as one will, between two given lines; as if that could not be done by the geometry of planes, &c.*" You might have left out *Tu vero* to seek an *Ego quidem.* But tell me, do you think that you can find two mean proportionals (which is less than

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as many as one will) by the geometry of planes? We shall see anon how you go about it. I never said it was impossible, and if you look upon the places cited by you more attentively, you will find yourself mistaken. But I say, the way to do it has not been yet found out, and therefore it may prove a solid problem for anything you know.

The fifth article you reject, because it citeth the corollary of the twenty-eighth article of the thirteenth chapter, where there is never a word to that purpose. But there is in the twenty-sixth article; which was my own fault, though you knew not but it might have been the printer's.

To the tenth you object for almost three leaves together, against these words of mine, *because*, in the sixth figure, *B C is to B F in triplicate proportion of C D to F E, therefore inverting, F E is to C D in triplicate proportion of B F to B C.* This you objected then. But now that I have taught you so much geometry, as to know *that of three quantities, beginning at the least, if the third be to the first in triplicate proportion of the second to the first, also by conversion the first to the second shall be in triplicate proportion of the first to the third;* if it were to do again, you would not object it.

My eleventh article you would allow for demonstrated, if my second had been demonstrated, upon which it dependeth. Therefore seeing your objections to that article are sufficiently answered, this article also is to be allowed.

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falsities you shall find in such follow-
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article, we shall then see when I come to the places where you object against them.

To the thirteenth article you object, "*that the same demonstration may be as well applied to a portion of any conoeides, parabolical, hyperbolical, elliptical, or any other, as to the portion of a sphere.*" By the truth of this let any man judge of your and my geometry. Your comparison of the sphere and conoeides, so far holds good, as to prove that the superficies of the conoeides is greater than the superficies of the cone described by the subtense of the parabolical, hyperbolical, or elliptical line. But when I come to say, that *the cause of the excess of the superficies of the portion of the sphere above the superficies of the cone, consists in the angle D A B, and the cause of the excess of the circle made upon the tangent A D, above the superficies of the same cone, consists in the magnitude of the same angle D A B,* how will you apply this to your conoeides? For suppose that the crooked line A B (in the seventh figure) were not an arch of a circle, do you think that the angles which it maketh with the subtense A B, at the points A and B, must needs be equal? Or if they be not, does the excess of the superficies of the circle upon A D above the superficies of the cone, or the excess of the superficies of the portion of the conoeides above the superficies of the same cone, consist in the angle D A B, or rather in the magnitude of the two unequal angles D A B, and A B A? You should have drawn some other crooked line, and made tangents to it through A and B, and you would presently have seen your error. See how you can answer this; for if this demonstration of

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mine stand firm, I may be bold to say, though the same be well demonstrated by Archimedes, that this way of mine is more natural, as proceeding immediately from the natural efficient causes of the effect contained in the conclusion; and besides, more brief and more easy to be followed by the fancy of the reader.

To the fourteenth article you say that I "*commit a circle in that I require in the fourth article the finding of two mean proportionals, and come not till now to show how it is to be done.*" Nor now neither. But in the mean time you commit two mistakes in saying so. The place cited by you in the fourth article is, in the Latin, p. 215, line 26, in the English, p. 255, line 24. Let any reader judge whether that be a requiring it, or a supposing it to be done; this is your first mistake. The second is, that in this place the proportion itself, which is, "*If these deficient figures could be described in a parallelogram exquisitely, there might be found thereby between any two lines given, as many mean proportionals as one would,*" is a theorem, upon supposition of these crooked lines exquisitely drawn; but you take it for a problem.

And proceeding in that error, you undertake the invention of two mean proportionals, using therein my first figure, which is of the same construction with the eighth that belongeth to this fourteenth article. Your construction is, "*Let there be taken in the diameter C A, (fig. 1) the two given lines, or two others proportional to them, as C H, C G, and their ordinate lines H F, G E (which by construction are in subtriplicate proportion of the intercepted diameters). These*

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lines will show the proportions which those four proportionals are to have." But how will you find the length of H F or G E, the ordinate lines? Will you not do it by so drawing the crooked line C F E, as it may pass through both the points F and E? You may make it pass through one of them, but to make it pass through the other, you must find two mean proportionals between G K and G L, or between H I and H P; which you cannot do, unless the crooked line be exactly drawn; which it cannot be by the geometry of planes. Go shew this demonstration of yours to Orontius, and see what he will say to it.

I am now come to an end of your objections to the seventeenth chapter, where you have an epiphonema not to be passed over in silence. But because you pretend to the demonstration of some of these propositions by another method in your *Arithmetica Infinitorum*, I shall first try whether you be able to defend those demonstrations as well as I have done these of mine by the method of motion.

The first proposition of your *Arithmetica Infinitorum* is this lemma: "*In a series, or row of quantities, arithmetically proportional, beginning at a point or cypher, as* 0, 1, 2, 3, 4, *&c. to find the proportion of the aggregate of them all, to the aggregate of so many times the greatest, as there are terms.*" This is to be done by multiplying the greatest into half the number of the terms.

The demonstration is easy. But how do you demonstrate the same? "*The most simple way,*" say you, "*of finding this and some other problems, is to do the thing itself a little way, and to*

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observe and compare the appearing proportions, and then by induction to conclude it universally." Egregious logicians and geometricians, that think an induction, without a numeration of all the particulars sufficient, to infer a conclusion universal, and fit to be received for a geometrical demonstration! But why do you limit it to the natural consecution of the numbers, 0, 1, 2, 3, 4, &c? Is it not also true in these numbers, 0, 2, 4, 6, &c. or in these, 0, 7, 14, 21, &c? Or in any numbers where the difference of nothing and the first number is equal to the difference between the first and second, and between the second and third, &c.? Again, are not these quantities, 1, 3, 5, 7, &c. in continual proportion arithmetical? And if you put before them a cypher thus, 0, 1, 3, 5, 7, do you think that the sum of them is equal to the half of five times seven? Therefore though your lemma be true, and by me (Chap. XIII. art. 5) demonstrated; yet you did not know why it is true; which also appears most evidently in the first proposition of your *Conic Sections*, where first you have this, "*that a parallelogram whose altitude is infinitely little, that is to say, none, is scarce anything else but a line.*" Is this the language of geometry? How do you determine this word *scarce?* The least altitude, is somewhat or nothing. If somewhat, then the first character of your arithmetical progression must not be a cypher; and consequently the first eighteen propositions of this your *Arithmetica Infinitorum* are all nought. If nothing, then your whole figure is without altitude, and consequently your understanding nought. Again, in the same proposition, you say thus:

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"*We will sometimes call those parallelograms rather by the name of lines than of parallelograms, at least when there is no consideration of a determinate altitude; but where there is a consideration of a determinate altitude (which will happen sometimes) there that little altitude shall be so far considered, as that being infinitely multiplied it may be equal to the altitude of the whole figure.*" See here in what a confusion you are when you resist the truth. When you consider no determinate altitude, that is no quantity of altitude, then you say your parallelogram shall be called a line. But when the altitude is determined, that is, when it is quantity, then you will call it a parallelogram. Is not this the very same doctrine which you so much wonder at and reprehend in me, in your objections to my eighth chapter, and your word *considered* used as I used it? It is very ugly in one that so bitterly reprehendeth a doctrine in another, to be driven upon the same himself by the force of truth when he thinks not on it. Again, seeing you admit in any case those infinitely little altitudes to be quantity, what need you this limitation of yours, "*so far forth as that by multiplication they may be made equal to the altitude of the whole figure?*" May not the half, the third, the fourth, or the fifth part, &c. be made equal to the whole by multiplication? Why could you not have said plainly, *so far forth as that every one of those infinitely little altitudes be not only something but an aliquot part of the whole?* So you will have an *infinitely little altitude*, that to say, *a point to be both nothing and something nd an aliquot part*. And all this proceeds from

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not understanding the ground of your profession. Well, the lemma is true. Let us see the theorems you draw from it. The first is (p. 3) "*that a triangle to a parallelogram of equal base and altitude is as one to two.*" The conclusion is true, but how know you that? "*Because,*" say you, "*the triangle consists as it were* [*as it were*, is no phrase of a geometrician] *of an infinite number of straight parallel lines.*" Does it so? Then by your own doctrine, which is, that "*lines have no breadth,*" the altitude of your triangle consisteth of an infinite number of no altitudes, that is of an infinite number of nothings, and consequently the area of your triangle has no quantity. If you say that by the parallels you mean infinitely little parallelograms, you are never the better; for if infinitely little, either they are nothing, or if somewhat, yet seeing that no two sides of a triangle are parallel, those parallels cannot be parallelograms. I see they may be counted for parallelograms by not considering the quantity of their altitudes in the demonstration. But you are barred of that plea, by your spiteful arguing against it in your *Elenchus*. Therefore this third proposition, and with it the fourth, is undemonstrated.

Your fifth proposition is, "*the spiral line is equal to half the circle of the first revolution.*" But what spiral line? We shall understand that by your construction, which is this: "*The straight line M A* [in your figure which I have placed at the end of the fifth lesson] *turned round* (*the point M remaining unmoved*) *is supposed to describe with its point A the circle A O A, whilst some point, in the same M A, whilst it goes about,*

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is supposed to be moved uniformly from M to A, describing the spiral line." This therefore, is the spiral line of Archimedes; and your proposition affirms it to be equal to the half of the circle A O A; which you perceived not long after to be false. But thinking it had been true, you go about to prove it, "*by inscribing in the circle an infinite multitude of equal angles, and consequently an infinite number of sectors, whose arches will therefore be in arithmetical proportion;*" which is true. "*And the aggregate of those arches equal to half the circumference A O A;*" which is true also. And thence you conclude "*that the spiral line is equal to half the circumference of the circle A O A;*" which is false. For the aggregate of that infinite number of infinitely little arches, is not the spiral line made by your construction, seeing by your construction the line you make is manifestly the spiral of Archimedes; whereas no number, though infinite, of arches of circles, how little soever, is any kind of spiral at all; and though you call it a spiral, that is but a patch to cover your fault, and deceiveth no man but yourself. Besides, you saw not how absurd it was, for you that hold a point to be absolutely nothing, to make an infinite number of equal angles (the radius increasing as the number of angles increaseth) and then to say, "*that the arches of the sectors whose angles they are, are as* 0, 1, 2, 3, 4, &c." For you make the first angle 0, and all the rest equal to it; and so make 0, 0, 0, 0, 0, &c. to be the same progression with 0, 1, 2, 3, 4, &c. The influence of this absurdity reacheth to the end of the eighteenth proposition. So many are therefore false,

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or nothing worth. And you needed not to wonder that the doctrine contained in them was omitted by Archimedes, who never was so senseless as to think a spiral line was compounded of arches of circles.

Your nineteenth proposition is this other lemma: "*In a series, or a row, of quantities, beginning from a point, or cypher, and proceeding according to the order of the square numbers, as* 0, 1, 4, 9, 16, *&c. to find what proportion the whole series hath to so many times the greatest.*" And you conclude "*the proportions to be that of* 1 *to* 3." Which is false, as you shall presently see. First, let the series of squares with the prefixed cypher, and under every one of them the greatest 4 be $\frac{0\,.\,1\,.\,4.}{4\,.\,4\,.\,4.}$ And you have for the sum of the squares 5, and for thrice the greatest 12, the third part whereof is 4. But 5 is greater than 4, by 1, that is, by one twelfth of 12; which quantity is somewhat, let it be called A. Again, let the row of squares be lengthened one term further, and the greatest set under every one of them as $\frac{0\,.\,1\,.\,4\,.\,9}{9\,.\,9\,.\,9\,.\,9}$. The sum of the squares is 14, and the sum of four times the greatest is 36, whereof the third part is 12. But 14 is greater than 12 by two unities, that is, by two twelfths of 12, that is, by 2 A. The difference therefore between the sum of the squares, and the sum of so many times the greatest square, is greater, when the cypher is followed by three squares, than when by but two. Again, let the row have five terms, as in these numbers $\frac{0\,.\,1\,.\,4\,.\,9\,.\,16}{16\,.\,16\,.\,16\,.\,16\,.\,16}$ with the greatest five times described, and the sum of the squares will be 30, the sum of all the greatest will be 80. The third part whereof is 26 $\frac{2}{3}$·

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But 30 is greater than 26 $\frac{2}{3}$ by 3$\frac{1}{3}$, that is, by three twelfths of twelve, and $\frac{1}{3}$ of a twelfth, that is, by 3$\frac{1}{3}$ A. Likewise in the series continued to six places with the greatest six times subscribed, as $\frac{0.\ 1.\ 4.\ 9.\ 16.\ 25.}{25.\ 25.\ 25.\ 25.\ 25.\ 25.}$ the sum of the squares is 55, and the sum of the greatest six times taken is 150, the third part whereof is 50. But 55 is greater than 50 by 5, that is, by five-twelfths of 12, that is by 5 A. And so continually as the row groweth longer, the excess also of the aggregate of the squares above the third part of the aggregate of so many times the greatest square, growing greater. And consequently if the number of the squares were infinite, their sum would be so far from being equal to the third part of the aggregate of the greatest as often taken, as that it would be greater than it by a quantity greater than any that can be given or named.

That which deceived you was partly this, that you think, as you do in your *Elenchus*, that these fractions $\frac{1}{12}$ $\frac{1}{18}$ $\frac{1}{24}$ $\frac{1}{30}$ $\frac{1}{36}$ &c. are proportions, as if $\frac{1}{12}$ were the proportion of one to twelve, and consequently $\frac{2}{12}$ double the proportion of one to twelve; which is as unintelligible as school-divinity ; and I assure you, far from the meaning of Mr. Ougthred in the sixth chapter of his *Clavis Mathematica*, where he says that 4 $\frac{3}{7}$ is the proportion of 31 to 7 ; for his meaning is, that the proportion of 4 $\frac{3}{7}$ to one, is the proportion of 31 to 7 ; whereas if he meant as you do, then 8$\frac{6}{7}$ should be double the proportion of 31 to 7. Partly also because you think (as in the end of the twentieth proposition) that if the proportion of the numerators of

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these fractions $\frac{1}{12}\,\frac{1}{18}\,\frac{1}{24}\,\frac{1}{30}\,\frac{1}{36}$ to their denominators decrease eternally, they shall so vanish at last as to leave the proportion of the sum of all the squares to the sum of the greatest so often taken, (that is, an infinite number of times), as one to three, or the sum of the greatest to the sum of the increasing squares, as three to one; for which there is no more reason than for four to one, or five to one, or any other such proportion. For if the proportions come eternally nearer and nearer to the subtriple, they must needs also come nearer and nearer to subquadruple; and you may as well conclude thence that the upper quantities shall be to the lower quantities as one to four, or as one to five, &c. as conclude they are as one to three. You can see without admonition, what effect this false ground of yours will produce in the whole structure of your *Arithmetica Infinitorum;* and how it makes all that you have said unto the end of your thirty-eighth proposition, undemonstrated, and much of it false.

The thirty-ninth is this other lemma: "*In a series of quantities beginning with a point or cypher, and proceeding according to the series of the cubic numbers, as* 0. 1. 8. 27. 64, *&c. to find the proportion of the sum of the cubes to the sum of the greatest cube, so many times taken as there be terms.*" And you conclude that "*they have a proportion of* 1 *to* 4;" which is false.

Let the first series be of three terms subscribed with the greatest $\frac{0\ .\ 1\ .\ 8}{8\ .\ 8\ .\ 8}$; the sum of the cubes is nine; the sum of all the greatest is 24; a quarter whereof is 6. But 9 is greater than 6 by three unities. An unity is something. Let it be there-

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fore A. Therefore the row of cubes is greater than a quarter of three times eight, by three A. Again, let the series have four terms, as $\frac{0\,.\,1\,.\,8\,.\,27}{27\,.\,27\,.\,27\,.\,27}$; the sum of the cubes is 36; a quarter of the sum of all the greatest is twenty-seven. But thirty-six is greater than twenty-seven by nine, that is, by 9 A. The excess therefore of the sum of the cubes above the fourth part of the sum of all the greatest, is increased by the increase of the number of terms. Again, let the terms be five, as $\frac{0\,.\,1\,.\,8\,.\,27\,.\,64}{64\,.\,64\,.\,64\,.\,64\,.\,64}$, the sum of the cubes is one hundred; the sum of all the greatest three hundred and twenty; a quarter whereof is eighty. But one hundred is greater than eighty by twenty, that is, by 20 A. So you see that this lemma also is false. And yet there is grounded upon it all that which you have of comparing parabolas and paraboloeides with the parallelograms wherein they are accommodated. And therefore though it be true, that the parabola is $\frac{2}{3}$, and the cubical paraboloeides $\frac{3}{4}$ of their parallelograms respectively, yet it is more than you were certain of when you referred me, for the learning of geometry, to this book of yours. Besides, any man may perceive that without these two lemmas (which are mingled with all your compounded series with their excesses) there is nothing demonstrated to the end of your book: which to prosecute particularly, were but a vain expense of time. Truly, were it not that I must defend my reputation, I should not have showed the world how little there is of sound doctrine in any of your books. For when I think how dejected you will be for the future, and how the grief of so much time irrecoverably lost, together

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with the conscience of taking so great a stipend, for mis-teaching the young men of the University, and the consideration of how much your friends will be ashamed of you, will accompany you for the rest of your life, I have more compassion for you than you have deserved. Your treatise of the *Angle of Contact*, I have before confuted in a very few leaves. And for that of your *Conic Sections*, it is so covered over with the scab of symbols, that I had not the patience to examine whether it be well or ill demonstrated.

Yet I observed thus much, that you find a tangent to a point given in the section by a diameter given; and in the next chapter after, you teach the finding of a diameter, which is not artificially done.

I observe also, that you call the *parameter* an imaginary line, as if the place thereof were less determined than the diameter itself; and then you take a mean proportional between the intercepted diameter, and its contiguous ordinate line, to find it. And it is true, you find it: but the parameter has a determined quantity, to be found without taking a mean proportional. For the diameter and half the section being given, draw a tangent through the vertex, and dividing the angle in the midst which is made by the diameter and tangent, the line that so divideth the angle, will cut the crooked line. From the intersection draw a line (if it be a parabola) parallel to the diameter, and that line shall cut off in the tangent from the vertex the parameter sought. But if the section be an ellipsis, or an hyperbole, you may use the same method, saving that the line drawn from the intersection

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must not be parallel, but must pass through the end of the transverse diameter, and then also it shall cut off a part of the tangent, which measured from the vertex is the parameter. So that there is no more reason to call the parameter an imaginary line than the diameter.

Lastly, I observe that in all this your new method of conics, you show not how to find the *burning points*, which writers call the *foci* and *umbilici* of the section, which are of all other things belonging to the conics most useful in philosophy. Why therefore were they not as worthy of your pains as the rest, for the rest also have already been demonstrated by others? You know the focus of the parabola is in the axis distant from the vertex a quarter of the parameter. Know also that the focus of an hyperbole, is in the axis, distant from the vertex, as much as the hypotenusal of a rectangled triangle, whose one side is half the transverse axis, the other side half the mean proportional between the whole transverse axis and the parameter, is greater than half the transverse axis.

The cause why you have performed nothing in any of your books (saving that in your *Elenchus* you have spied a few negligences of mine, which I need not be ashamed of) is this, that you understood not what is *quantity*, *line*, *superficies*, *angle*, and *proportion;* without which you cannot have the science of any one proposition in geometry. From this one and first definition of Euclid, "*a point is that whereof there is no part*," understood by Sextus Empiricus, as you understand it, that is to say misunderstood, Sextus Empiricus had utterly destroyed most of the rest, and demonstra-

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ted, that in geometry there is no science, and by that means you have betrayed the most evident of the sciences to the sceptics. But as I understand it for *that whereof no part is reckoned*, his arguments have no force at all, and geometry is redeemed. If a line have no latitude, how shall a cylinder rolling on a plane, which it toucheth not but in a line, describe a superficies? How can you affirm that any of those things can be without quantity, whereof the one may be greater or less than the other? But in the common contact of divers circles the external circle maketh with the common tangent a less angle of contact than the internal. Why then is it not quantity? An angle is made by the concourse of two lines from several regions, concurring, by their generation, in one and the same point. How then can you say the angle of contact is no angle? One measure cannot be applicable at once to the angle of contact, and angle of conversion. How then can you infer, if they be both angles, that they must be homogeneous? Proportion is the relation of two quantities. How then can a quotient or fraction, which is quantity absolute, be a proportion? But to come at last to your *Epiphonema*, wherein, though I have perfectly demonstrated all those propositions concerning the proportion of parabolasters to their parallelograms, and you have demonstrated none of them (as you cannot now but plainly see), but committed most gross paralogisms, how could you be so transported with pride, as insolently to compare the setting of them forth as mine, to the act of him that steals a horse, and comes to the gallows for it. You have read, I think, of the gallows set up by Haman. Remember therefore also who was hanged upon it.

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After your dejection I shall comfort you a little, a very little, with this, that whereas this eighteenth chapter containeth two problems, one, "*the finding of a straight line equal to the crooked line of a semi-parabola*;" the other, "*the finding of straight lines equal to the crooked lines of the parabolasters, in the table of the third article of the seventeenth chapter*;" you have truly demonstrated that they are both false; and another hath also demonstrated the same another way. Nevertheless, the fault was not in my method, but in a mistake of one line for another and such as was not hard to correct; and is now so corrected in the English as you shall not be able (if you can sufficiently imagine motions) to reprehend. The fault was this, that in the triangles which have the same base and altitude with the parabola and parabolaster, I take for designation of the mean uniform impetus, a mean proportional, in the first figure, between the whole diameter and its half, and, in the second figure, a mean proportional between the whole diameter and its third part; which was manifestly false, and contrary to what I had shown in the sixteenth chapter. Whereas I ought to have taken the half of the base, as now I have done, and thereby exhibited the straight lines equal to those crooked lines, as I undertook to do. Which error therefore proceeded not from want of skill, but from want of care; and what I promised (as bold as you say the promise was), I have now performed.

The rest of your exceptions to this chapter, are to these words in the end: "*There be some that* *say, that though there be equality between a straight and crooked line, yet now, they say, after the fall of Adam, it cannot be found without the especial help of divine grace.*" And you say you think there be none that say so. I am not bound to tell you who they are. Nevertheless, that other men may see the spirit of an ambitious part of the clergy, I will tell you where I read it. It is in the *Prolegomena* of Lalovera, a Jesuit, to his Quadrature of the Circle, p. 13 and 14, in these words: "*Quamvis circuli tetragonismus sit* φύσει *possibilis, an tamen etiam* πρός ἡμᾶς, *hoc est, post Adæ lapsum homo ejus scientiam absque speciali divinæ gratiæ auxilio, possit comparare, jure merito inquirunt theologi, pronunciantque; hanc veritatem tanta esse caligine involutam ut illam videre nemo possit, nisi ignorantiæ ex primi parentis prævaricatione propagatas tenebras indebitus divinæ lucis radius dissipet; quod verissimum esse sentio.*" Wherein I observed that he, supposing he had found that quadrature, would have us believe it was not by the ordinary and natural help of God (whereby one man reasoneth, judgeth and remembereth better than another), but by a special (which must be a supernatural) help of God, that he hath given to him of the order of Jesus above others that have attempted the same in vain. Insinuating thereby, as handsomely as he could, a special love of God towards the Jesuits. But you taking no notice of the word *special*, uld have men think I held, that human sciences t be acquired without any help of God. And upon proceed in a great deal of ill language end of your objections to this chapter. But

I shall take notice of your manners for altogether in my next lesson.

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At the nineteenth chapter you see not, you say, the method. Like enough. In this chapter I consider not the cause of reflection, which consisteth in the resistance of bodies natural; but I consider the consequences, arising from the supposition of the equality of the angle of reflection, to that of incidence; leaving the causes both of reflection, and of refraction, to be handled together in the twenty-fourth chapter. Which method, think what you will, I still think best.

Secondly, you say I define not here, but many chapters after, what an angle of incidence, and what an angle of reflection is. Had you not been more hasty than diligent readers, you had found that those definitions of the angle of incidence, and of reflection, were here set down in the first article, and not deferred to the twenty-fourth. Let not therefore your own oversight be any more brought in for an objection.

Thirdly, you say there is no great difficulty in the business of this chapter. It may be so, now it is down; but before it was done, I doubt not but you that are a professor would have done the same, as well as you have done that of the *Angle of Contact*, or the business of your *Arithmetica Infinitorum*. But what a novice in geometry would have done I cannot tell.

To the third, fourth, and fifth article, you object a want of determination; and show it by instance, as to the third article. But what those determinations should be, you determine not, because you could not. The words in the third

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article, are first these, *if there fall two straight lines parallel, &c.* which is too general. It should be, *if there fall the same way two straight lines parallel, &c.* Next these, *their reflected lines produced inwards shall make an angle, &c.* This also is too general. I should have said, *their reflected lines produced inwards, if they meet within, shall make an angle, &c.* Which done, both this article and the fourth and fifth are fully demonstrated. And without it, an intelligent reader had been satisfied, supplying the want himself by the construction.

To the eighth, you object only the too great length and labour of it, because you can do it a shorter way. Perhaps so now, as being easy to shorten many of the demonstrations both of Euclid, and other the best geometricians that are or have been. And this is all you had to say to my nineteenth chapter. Before I proceed, I must put you in mind that these words of yours, "*adducis malleum, ut occidas muscam,*" are not good Latin, *malleum affers, malleum adhibes, malleo uteris*, are good. When you speak of bringing bodies animate, *ducere* and *adducere* are good, for there *to bring*, is *to guide or lead.* And of bodies inanimate, *adducere* is good for *attrahere*, which is to draw to. But when you bring a hammer, will you say *adduco malleum, I lead a hammer?* A man may lead another man, and a ninny may be said to lead another ninny, but not a hammer. Never- ess, I should not have thought fit to reprehend ult upon this occasion in an Englishman, take notice of it, but that I find you in aces nibbling, but causelessly, at my Latin.

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Concerning the twentieth chapter, before I answer to the objections against the propositions themselves, I must answer to the exception you first take to these words of mine, "*Quæ de dimensione circuli et angulorum pronuntiata sunt tanquam exactè inventa, accipiat lector tanquam dicta problematicè.* To which you say thus: "*We are wont in geometry to call some propositions theorems, others problems, &c. of which a theorem is that wherein some assertion is propounded to be proved; a problem that wherein something is commanded to be done.*" Do you mean *to be done,* and not proved? By your favour, a problem in all ancient writers signifies no more but a proposition uttered, to the end to have it, by them to whom it is uttered, examined whether it be true or not true, faisable or not faisable; and differs not amongst geometricians from a theorem but in the manner of propounding. For this proposition, *to make an equilateral triangle,* so propounded they call a problem. But if propounded thus: *If upon the ends of a straight line given be described two circles, whose radius is the same straight line, and there be drawn from the intersection of the circles to their two centres, two straight lines, there will be made an equilateral triangle,* then they call it a theorem; and yet the proposition is the same. Therefore these words, *accipiat lector tanquam dicta problematicè* signify plainly this, that I would have the reader, take for propounded to him to examine, whether from my construction the quadrature of the circle can be truly inferred or not; and this is not to bid him, as you interpret it, to square the circle. And if

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you believe that *problematicè* signifies probably, you have been very negligent in observing the sense of the ancient Greek philosophers in the word problem. Therefore your *solemus in geometria*, &c. is nothing to the purpose; nor had it been though you had spoken more properly, and said *solent*, leaving out yourselves.

My first article hath this title, "*from a false supposition, a false quadrature of the circle.*" Seeing therefore you were resolved to show where I erred, you should have proved either that the supposition was true, and the conclusion falsely inferred, or contrarily, that though the supposition be false, yet the conclusion is true; for else you object nothing to my geometry, but only to my judgment, in thinking fit to publish it; which nevertheless you cannot justly do, seeing it was likely to give occasion to ingenious men (the practice of it being so accurate to sense) to inquire wherein the fallacy did consist. And for the problem as it was first printed, but never published, and consequently ought to have passed for a private paper stolen out of my study, your public objecting against it (in the opinion of all men that have conversed so much with honest company as to know what belongs to civil conversation), was sufficiently barbarous in divines. And seeing you knew I had rejected that proposition, it was but a poor ambition to take wing as you thought to do, like beetles from my egestions. But let that be as it will, you will think strange now I should resume, and make good, at least against your objection, that very same proposition. So much of the figure as is needful you will find noted with the

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same letters, and placed at the end of this fifth lesson. Wherein let B I, be an arch not greater than the radius of the circle, and divided into four equal parts, in L, N, O. Draw S N, the sine of the arch B N, and produce it to T, so as S T be double to S N, that is, equal to the chord B I. Draw likewise *a* L, the sine of the arch B L, and produce it to *c*, so as *a c* be quadruple to *a* L, that is, equal to the two chords B N, N I. Upon the centre N with the radius N I, draw the arch I *d*, cutting B U the tangent in *d*. Then will B N produced cut the arch I *d*, in the midst at *o*. In the line B S produced take S *b*, equal to B S; then draw and produce *b* N, and it will fall on the point *d*. And B *d*, S T, will be equal; and *d* T joined and produced will fall upon *o*, the midst of the arch I *d*. Join I T, and produce it to the tangent B U in U. I say, that the straight line I T U shall pass through *c*. For seeing B S, S *b*, are equal, and the angle at S a right angle, the straight lines B N, and *b* N, are also equal, and the triangles B N *b*, *d* N *o* like and equal; and the lines *d* T, T *o* equal. Draw *o i* parallel to *d* U, cutting I U in *i*; and the triangles *d* T U, *o* T *i* will also be like and equal. Produce S T to the arch *d o* I in *e*, and produce it further to *f*, so that the line *e f* be equal to T *e*; and then S *f* will be equal to *a c*. Therefore *f c* joined will be parallel to B S. In *c f* produced take *f g* equal to *c f*; and draw *g m* parallel to *d* U, cutting I U in *m*, and *d o* in *n*; and let the intersection of the two lines *a c* and *d o* be in *r*; which being done, the triangles *m n* T, *r c* T will be like and equal. Therefore *m n* and *r c* are equal; and consequently

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the straight line I *m* T U shall pass through *c*. Dividing therefore *a c* in the midst at *t*, and S N in the midst at *l*, and joining *t* N, L *l*, the lines L *l*, *t* N, and *c* T produced, will all meet in one and the same point of B S produced; suppose at *q*. Therefore the point *q* being given by the two known points T and I, the lines drawn from *q* through equal parts of the sine of the arch B I, (for example through the points P, Q, R, of the sine M I), shall cut off equal arches, as B L, L N, N O, O I. And this is enough to make good that problem, as to your objection.

The straight line therefore B U, for any thing you have said, is proved equal to the arch B I, and the division of any angle given into any proportion given, the quadrature of any sector, and the construction of any equilateral polygon is also given. And though in this also I should have erred, yet it cannot be denied but that I have used a more natural, a more geometrical, and a more perspicuous method in the search of this so difficult a problem, than you have done in your *Arithmetica Infinitorum*. For though it be true that the aggregate of all the mean proportionals between the radius, together with an infinitely little part of the same, and the radius wanting an infinitely little part of the same; and again, between the radius, together with two infinitely little parts, and the radius wanting two infinitely little parts, and so on eternally, will be equal to the quadrant (a thing which every mean geometrician knew before); yet it was absurd to think those means could be calculated in numbers by interpoling of a symbol; especially when you make that symbol to stand for

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a number neither true nor surd; as if there were a number that could neither be uttered in words, nor not be uttered in words. For what else is surd, but that which cannot be spoken?

To the fifth article, though your discourse be long, you object but two things. One is, that "*Whereas the spiral of Archimedes is made of two motions, one straight, the other circular, both uniform, I taking the motion compounded of them both for one of those that are compounded, conclude falsely, that the generation of the spiral is like to the generation of the parabola.*" What heed you use to take in your reprehensions, appears most manifestly in this objection. For I say in that demonstration of mine, that *the velocity of the point A in describing the spiral increaseth continually in proportion to the times.* For seeing it goes on uniformly in the semidiameter, it is impossible it should not pass into greater and greater circles, proportionally to the times, and consequently it must have a swifter and swifter motion circular, to be compounded with the uniform motion in every point of the radius as it turneth about. This objection therefore is nothing but an effect of a will, without cause, to contradict.

The other objection is, that "*Granting all to be true hitherto, yet because it depends upon the finding of a straight line equal to a parabolical line in the eighteenth chapter, where I was deceived, I am also deceived here.*" True. But because in the eighteenth chapter of this English edition I have found a straight line equal to the spiral line of Archimedes. I must here put you in mind that by these words in your objections to the

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fifth article at your number two, *Quatenus verum est, etc., we have demonstrated prop.* 10, 11, 13, *Arithmetica Infinitorum;* you make it appear that you thought your spiral (made of arches or circles) was the true spiral of Archimedes; which is fully as absurd as the quadrature of Joseph Scaliger, whose geometry you so much despise.

To the sixth article, which is a digression concerning the analytics of geometricians, you deny *that the efficient cause of the construction ought to be contained in the demonstration.* As if any problem could be known to be truly done, otherwise than by knowing first how, that is to say, by what efficient cause, and in what manner, it is to be done. Whatsoever is done without that knowledge, cannot be demonstrated to be done; as you see in your computation of the parabola, and paraboloeides, in your *Arithmetica Infinitorum.*

And whereas I said that *the ends of all straight lines drawn from a straight line, and passing through one and the same point, if their parts be proportional, shall be in a straight line;* is true and accurate; as also, *if they begin in the circumference of a circle, they shall also be in the circumference of another circle.* And so is this: *if the proportion be duplicate, they shall be in a parabola.* All this I say is true and accurately spoken. But this was no place for the demonstration of it. Others have done it. And I perceive by that you put in by parenthesis *("Intelligis credo inter duas peripherias concentricas")* that you understand not what I mean.

Hitherto reach your objections to my geometry: for the rest of your book, it containeth nothing

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but a collection of lies, wherewith you do what you can, to extenuate as vulgar, and disgrace as false, that which followeth, and to which you have made no special objection.

I shall therefore only add in this place concerning your *Analytica per Potestates,* that it is no art. For the rule, both in Mr. Ougthred, and in Des Cartes, is this: " *When a problem or question is propounded, suppose the thing required done, and then using a fit ratiocination, put A or some other vowel for the magnitude sought.*" How is a man the better for this rule without another rule, how to know when the ratiocination is fit? There may therefore be in this kind of analysis more or less natural prudence, according as the analyst is more or less wise, or as one man in choosing of the unknown quantity with which he will begin, or in choosing the way of the consequences which he will draw from the hypothesis, may have better luck than another. But this is nothing to art. A man may sometimes spend a whole day in deriving of consequences in vain, and perhaps another time solve the same problem in a few minutes.

I shall also add, that symbols, though they shorten the writing, yet they do not make the reader understand it sooner than if it were written in words. For the conception of the lines and figures (without which a man learneth nothing) must proceed from words either spoken or thought upon. So that there is a double labour of the mind, one to reduce your symbols to wŏrds, which are also symbols, another to attend to the ideas which they signify. Besides, if you but consider how none of

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the ancients ever used any of them in their published demonstrations of geometry, nor in the books of arithmetic, more than for the roots an potestates themselves; and how bad success yo have had yourself in the unskilful using of them, you will not, I think, for the future be so much in love with them as to demonstrate by them that first part you promise of your *Opera Mathematica.* In which, if you make not amends for that which you have already published, you will much disgrace those mathematicians you address your epistles to, or otherwise have commended; as also the Universities, as to this kind of learning, in the sight of learned men beyond sea. And thus having examined your pannier of Mathematics, and finding in it no knowledge, neither of quantity, nor of measure, nor of proportion, nor of time, nor of motion, nor of any thing, but only of certain characters, as if a hen had been scraping there; I take out my hand again, to put it into your other pannier of theology, and good manners. In the mean time I will trust the objections made by you the astronomer (wherein there is neither close reasoning, nor good style, nor sharpness of wit, to impose upon any man) to the discretion of all sorts of readers.

OF MANNERS.

TO THE SAME EGREGIOUS PROFESSORS OF THE MATHEMATICS IN THE UNIVERSITY OF OXFORD.

LESSON VI.

LESSON VI.
Of manners.

HAVING in the precedent lessons maintained the truth of my geometry, and sufficiently made appear that your objections against it are but so many errors of your own, proceeding from misunderstanding of the propositions you have read in Euclid, and other masters of geometry; I leave it to your consideration to whom belong, according to your own sentence, the unhandsome attributes you so often give me upon supposition, that you yourselves are in the right, and I mistaken; and come now to purge myself of those greater accusations which concern my manners. It cannot be expected that there should be much science of any kind in a man that wanteth judgment; nor judgment in a man that knoweth not the manners due to a public disputation in writing; wherein the scope of either party ought to be no other than the examination and manifestation of the truth. For whatsoever is dded of contumely, either directly or *scomma- ically*, is want of charity and uncivil, unless it be one by way of reddition from him that is first rovoked to it. I say unless it be by way of red- tion; for so was the judgment given by the nperor Vespasian in a quarrel between a senator nd a knight of Rome which had given him ill nguage. For when the knight had proved that ne first ill language proceeded from the senator,

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the emperor acquitted him in these words: "*Maledici senatoribus non oportere; remaledicere, fas et civile esse.*" Nevertheless, now-a-days, uncivil words are commonly and bitterly used by all that write in matter of controversy, especially in divinity, excepting now and then such writers as have been more than ordinarily well bred, and have observed how heinous and hazardous a thing such contumely is amongst some sorts of men, whether that which is said in disgrace be true or false. For evil words by all men of understanding are taken for a defiance, and a challenge to open war. But that you should have observed so much, who are yet in your mother's belly, was not a thing to be much expected.

The faults in manners you lay to my charge are these: 1. *Self-conceit.* 2. *That I will be very angry with all men that do not presently submit to my dictates.* 3. *That I had my doctrine concerning Vision, out of papers which I had in my hands of Mr. Warner's.* 4. *That I have injured the universities.* 5. *That I am an enemy to religion.* These are great faults; but such as I cannot yet confess. And therefore I must, as well as I can, seek out the grounds upon which you build your accusation. Which grounds (seeing you are not acquainted with my conversation) must be either in my published writings, or reported to you by honest men, and without suspicion of interest in reporting it. As for my self-conceit and ostentation, you shall find no such matter in my writings. That which you allege from thence is first, that in the epistle dedicatory I say of my book *De Corpore*, "*though it be little, yet it is*

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full; and if good may go for great, great enough." When a man presenting a gift great or small to his betters, adorneth it the best he can to make it the more acceptable; he that thinks this to be ostentation and self-conceit, is little versed in the common actions of human life. And in the same epistle, where I say of civil philosophy: "*It is no ancienter than my book De Cive;*" these words are added: "*I say it provoked, and that my detractors may see they lose their labour.*" But that which is truly said, and upon provocation, is not boasting, but defence. A short sum of that book of mine, now publicly in French, done by a gentleman I never saw, carrieth the title of *Ethics Demonstrated.* The book itself translated into French, hath not only a great testimony from the translator Sorberius, but also from Gassendus, and Mersennus, who being both of the Roman religion had no cause to praise it, or the divines of England have no cause to find fault with it. Besides, you know that the doctrine therein contained is generally received by all but those of the clergy, who think their interest concerned in being made subordinate to the civil power; whose testimonies therefore are invalid. Why therefore, if I commend it also against them that dispraise it publicly, do you call it boasting? "*You have heard,*" you say, "*that I had promised the quadrature of the circle, &c.*" You heard then that which was not true. I have been asked sometimes, by such as saw the figure before me, what I was doing, and I was not afraid to say I was seeking for the solution of that problem; but not that I had done it. And afterwards being asked of the success, I have

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said, I thought it done. This is not boasting; and yet it was enough, when told again, to make a fool believe it was boasting. But you, the astronomer, in the epistle before your philosophical essay, say "*You had a great expectation of my philosophical and mathematical works, before they were published.*" It may be so. Is that my fault? Can a man raise a great expectation of himself by boasting? If he could, neither of you would be long before you raised it of yourselves; saving that what you have already published, has made it now too late. For I verily believe there was never seen worse reasoning than in that philosophical essay; which any judicious reader would believe proceeded from a prevaricator, rather than from a man that believed himself; nor worse principles, than those in your books of Geometry. The expectation of that which should be written by me, was raised partly by the *Cogitata Physica-Mathematica* of Mersennus, wherein I am often named with honour; and partly by others with whom I then conversed in Paris, without any ostentation. That no man has a great expectation of any thing that shall proceed from either of you two, I am content to let it be your praise.

Another argument of my self-conceit, you take from my contempt of the writers of metaphysics and school-divinity. If that be a sign of self-conceit, I must confess I am guilty; and if your geometry had then been published, I had contemned that as much. But yet I cannot see the consequence (unless you lend me your better logic) from despising insignificant and absurd language, to self-conceit.

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And again, in your *Vindiciæ Academiarum*, you put for boasting, that in my *Leviathan*, page 331, I would have *that book by entire sovereignty imposed upon the Universities;* and in my *Review*, p. 713, that I say of my *Leviathan*, "*I think it may be profitably printed, and more profitably taught in the University*." The cause of my writing that book, was the consideration of what the ministers before, and in the beginning of, the civil war, by their preaching and writing did contribute thereunto. Which I saw not only to tend to the abatement of the then civil power, but also to the gaining of as much thereof as they could (as did afterwards more plainly appear) unto themselves. I saw also that those ministers, and many other gentlemen who were of their opinion, brought their doctrines against the civil power from their studies in the Universities. Seeing therefore that so much as could be contributed to the peace of our country, and the settlement of sovereign power without any army, must proceed from teaching; I had reason to wish, that civil doctrine were truly taught in the Universities. And if I had not thought that mine was such, I had never written it. And having written it, if I had not recommended it to such as had the power to cause it to be taught, I had written it to no purpose. To me therefore that never did write anything in philosophy to show my wit, but, as I thought at least, to benefit some part or other of mankind, it was very necessary to commend my doctrine to such men as should have the power and right to regulate the Universities. I say my doctrine; I say not my *Leviathan*. For wiser men may so digest the same

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doctrine as to fit it better for a public teaching. But as it is, I believe it hath framed the minds of a thousand gentlemen to a conscientious obedience to present government, which otherwise would have wavered in that point. This therefore was no vaunting, but a necessary part of the business I took in hand. You ought also to have considered, that this was said in the close of that part of my book which concerneth policy merely civil. Which part, if you, the astronomer, that now think the doctrine unworthy to be taught, were pleased once to honour with praises printed before it, you are not very constant nor ingenuous. But whether you did so or not, I am not certain, though it was told me for certain. If it were not you, it was somebody else whose judgment has as much weight at least as yours.

And for anything you have to say from your own knowledge, I remember not that I ever saw either of your faces. Yet you, the professor of geometry, go about obliquely to make me believe that Vindex hath discoursed with me, once at least, though I remember it not. I suppose it therefore true; but this I am sure is false, that either he or any man living did ever hear me brag of my science, or praise myself, but when my defence required it. Perhaps some of our philosophers that were at Paris at the same time, and acquainted with the same learned men that I was acquainted with, might take for bragging the maintaining of my opinions, and the not yielding to the reasons alledged against them. If that be ostentation, they tell you the truth. But you that are so wise should have considered, that even such men as

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ɔfess philosophy are carried away with the pas-
ns of emulation and envy (the sole ground of
s your accusation) as well as other men, and
tanced in yourselves. And this is sufficient to
ıke off your aspersions of ostentation and self-
ıceit. For if I added, that my acquaintance
ow that I am naturally of modest rather than of
asting speech, you will not believe it; because
u distinguish not between that which is said
on provocation, and that which is said without
ovocation, from vain glory.

The next accusation is: "*That I will be very
gry with all men that do not presently submit
my dictates; and that for advancing the repu-
tion of my own skill, I care not what unworthy
flections I cast on others.*" This is in the epistle
aced before the *Vindiciæ Academiarum*, subscri-
d by N S, as the plain song for H D in the rest
the book to descant upon. I know well enough
e authors' names; and am sorry that N S has
nt his name to be abused to so ill a purpose.
ut how does this appear? What argument,
hat witness is there of it? You offer none;
ɔr am I conscious of any. I begin to suspect
ıce you, the professor of geometry, have in
ɔur objections to the twentieth chapter these
ords concerning "*Vindex, ocularis ille testis de
ıo hic agitur, erat, ni fallor, ille ipse,*"—that
index himself, in other company, has bestowed a
sit on me. Seeing you will have me believe it,
t it be so; and, as it is likely, not long after my
turn into England. At which time (for the
putation, it seems, I had gotten by my boasting)
vers persons that professed to love philosophy

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and mathematics, came to see me; and some of them to let me see them, and hear and applaud what they applauded in themselves. I see now it hath happened to me with Vindex, as it happened to Dr. Harvey with Moranus. Moranus, a jesuit, came out of Flanders hither, especially, as he says, to see what learned men in divinity, ethics, physics, and geometry, were here yet alive, to the end that by discoursing with them in these sciences, he might correct either his own, or their errors. Amongst others he was brought, he says, to that most civil and renowned old man Dr. Harvey. That is very well. And in good earnest if he had made good use of the time which was very patiently afforded him, he might have learned of him (or of no man living) very much knowledge concerning the circulation of the blood, the generation of living creatures, and many other difficult points of natural philosophy. And if he had had anything in him but common and childish learning, he could have showed it nowhere more to his advantage, than before him that was so great a judge of such matters. But what did he? That precious time (which was but little, because he was to depart again presently for Flanders) he bestowed wholly in venting his own childish opinions, not suffering the Doctor scarce to speak; losing thereby the benefit he came for, and discovering that he came not to hear what others could say, but to show to others how learned he was himself already. Why
take so little time, and so mispend it?
ned he not again? But when he had
is time, and found (though patiently
ard) he was not much admired, he

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took occasion, writing against me, to be revenged of Dr. Harvey, by slighting his learning publicly; and tells me that his learning was only experiments; which he says I say have no more certainty than civil histories. Which is false. My words are: "*Ante hos nihil certi in physica erat præter experimenta cuique sua, et historias naturales, si tamen et hæ dicendæ certæ sint, quæ civilibus historiis certiores non sunt.*" Where I except expressly from uncertainty the experiments that every man maketh to himself. But you see the near cut, by which vain glory joined with ignorance passeth quickly over to envy and contumely.

Thus it seems by your own confession I was used by Vindex. He comes with some of my acquaintance in a visit. What he said I know not, but if he discoursed then, as in his philosophical essay he writeth, I will be bold to say of myself, I was so far from morosity, or, to use his phrase, from being tetrical, as I may very well have a good opinion of my own patience. And if there passed between us the discourse you mention in your *Elenchus*, page 116, it was an incivility in him so great, that without great civility I could not have abstained from bidding him be gone. That which passed between us you say was this: "*I complained that whereas I made sense, nothing but a perception of motion in the organ, nevertheless, the philosophy schools through all Europe, led by the text of Aristotle, teach another doctrine, namely, that sensation is performed by species.*" This is a little mistaken. For I do glory, not complain, that whereas all the Universities of Europe hold sensation to proceed from species, I

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hold it to be a perception of motion in the organ. The answer of Vindex, you say, was: "*That the other hypothesis, whereby sense was explicated by the principles of motion, was commonly admitted here before my book came out, as having been sufficiently delivered by Des Cartes, Gassendus, and Sir Kenelm Digby, before I had published anything in this kind.*" This then, it seems, was it that made me angry. Truly I remember not an angry word that ever I uttered in all my life to any man that came to see me, though some of them have troubled me with very impertinent discourse; and with those that argued with me, how impertinently soever, I always thought it more civility to be somewhat earnest in the defence of my opinion, than by obstinate and affected silence to let them see I contemned them, or hearkened not to what they said. If I were earnest in making good, that the manner of sensation by such motion as I had explicated in my *Leviathan*, is in none of the authors by him named, it was not anger, but a care of not offending him, with any sign of the contempt which his discourse deserved. But it was incivility in him to make use of a visit, which all men take for a profession of friendship, to tell me that that which I had already published for my own, was found before by Des Cartes, Gassendus, and Sir Kenelm Digby. But let any man read Des Cartes; he shall find, that he attributeth

on at all to the object of sense, but an
to action, which inclination no man can
hat it meaneth. And for Gassendus,
elm Digby, it is manifest by their wri-
their opinions are not different from

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that of Epicurus, which is very different from mine. Or if these two, or any of those I conversed with at Paris, had prevented me in publishing my own doctrine, yet since it was there known, and declared for mine by Mersennus in the preface to his *Ballistica* (of which the three first leaves are employed wholly in the setting forth of my opinion concerning sense, and the rest of the faculties of the soul) they ought not therefore to be said to have found it out before me. And consequently this answer which you say was given me by Vindex was nothing else but untruth and envy; and, because it was done by way of visit, incivility. But you have not alleged, nor can allege, any words of mine, from which can be drawn that I am so angry as you say I am with those that submit not to my dictates. Though the discipline of the University be never so good; yet certainly this behaviour of yours and his are no good arguments to make it thought so. But you the professor of geometry, that out of my words spoken against Vindex in my twentieth chapter, argue my angry humour, do just as well, as when (in your *Arithmetica Infinitorum*) from the continual increase of the excess of the row of squares above the third part of the aggregate of the greatest, you conclude they shall at last be equal to it. For though you knew that Vindex had given me first the worst words that possibly can be given, yet you would have that return of mine to be a demonstration of an angry humour; not then knowing what I told you even now in the beginning of this lesson, of the sentence given by Vespasian. But to this point I shall speak again hereafter.

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Your third accusation is: "*That I had my doctrine of vision, which I pretended to be my own, out of papers which I had a long time in my hands of Mr. Warner's.*" I never had sight of Mr. Warner's papers in all my life, but that of *Vision by Refraction* (which by his approbation I carried with me to Park, and caused it to be printed under his own name, at the end of Mersennus his *Cogitata Physico-Mathematica*, which you may have there seen, and another treatise of the proportions of alloy in gold and silver coin; which is nothing to the present purpose. In all my conversation with him, I never heard him speak of anything he had written, or was writing, *De penicillo optico*. And it was from me that he first heard it mentioned that light and colour were but fancy. Which he embraced presently as a truth, and told me it would remove a rub he was then come to in the discovery of the place of the image. If after my going hence he made any use of it (though he had it from me, and not I from him), it was well done. But wheresoever you find my principles, make use of them, if you can, to demonstrate all the symptoms of vision; and I will do (or rather have done and mean to publish) the same; and let it be judged by that, whether those principles be of mine, or other men's invention. I give you time enough, and this advantage besides, that much of my optics hath been privately read by others. For I never refused to lend my papers to my friends, as knowing it to be a thing of no prejudice to the advancement of philosophy, though it be, as I have found it since, some prejudice to the advancement of my own reputation in those

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sciences; which reputation I have always postposed to the common benefit of the studious.

You say further (you the geometrician) that I had the proposition of the spiral line equal to a parabolical line from Mr. Robervall: true. And if I had remembered it, I would have taken also his demonstration; though if I had published his, I would have suppressed mine. I was comparing in my thoughts those two lines, spiral and parabolical, by the motions wherewith they were described; and considering those motions as uniform, and the lines from the centre to the circumference, not to be little parallelograms, but little sectors, I saw that to compound the true motion of that point which described the spiral, I must have one line equal to half the perimeter, the other equal to half the diameter. But of all this I had not one word written. But being with Mersennus and Mr. Robervall in the cloister of the convent, I drew a figure on the wall, and Mr. Robervall perceiving the deduction I made, told me that since the motions which make the parabolical line, are one uniform, the other accelerated, the motions that make the spiral must be so also; which I presently acknowledged; and he the next day, from this very method, brought to Mersennus the demonstration of their equality. And this is the story mentioned by Mersennus, prop. 25, corol. 2, of his *Hydraulica;* which I know not who hath most magnanimously interpreted to you in my disgrace.

The fourth accusation is: "*That I have injured the Universities.*" Wherein? First, "*In that I would have the doctrine of my Leviathan by en-*

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tire sovereignty be imposed on them." You often upbraid me with thinking well of my own doctrine; and grant by consequence, that I thought this doctrine good ; I desired not therefore that anything should be imposed upon them, but what (at least in my opinion) was good both for the Commonwealth and them. Nay more, I would have the state make use of them to uphold the civil power, as the Pope did to uphold the ecclesiastical. Is it not absurdly done to call this an injury ? But to question, you will say, whether the civil doctrine there taught be such as it ought to be, or not, is a disgrace to the Universities. If that be certain, it is certain also that those sermons and books, which have been preached and published, both against the former and the present government, directly or obliquely, were not made by such ministers and others as had their breeding in the Universities; though all men know the contrary. But the doctrine which I would have to be taught there, what is it? It is this: "*That all men that live in a Commonwealth, and receive protection of their lives and fortunes from the supreme governor thereof, are reciprocally bound, as far as they are able, and shall be required, to protect that governor.*" Is it, think you, an unreasonable thing to impose the teaching of such doctrine upon the Universities? Or will you say they taught it before, when you know that so many men which came from the Universities to preach to the people, and so many others that were not ministers, did stir the people up to resist the then supreme civil power? And was it not truly therefore said, that the Universities receiving their discipline from the

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authority of the pope, were the shops and operatories of the clergy? Though the competition of the papal and civil power be taken now away, yet the competition between the ecclesiastical and the civil power hath manifestly enough appeared very lately. But neither is this an upbraiding of an University (which is a corporation or body artificial), but of particular men, that desire to uphold the authority of a Church, as of a distinct thing from the Commonwealth. How would you have exclaimed, if, instead of recommending my *Leviathan* to be taught in the Universities, I had recommended the erecting of a new and lay-university, wherein lay-men should have the reading of physics, mathematics, moral philosophy, and politics, as the clergy have now the sole teaching of divinity? Yet the thing would be profitable, and tend much to the polishing of man's nature, without much public charge. There will need but one house, and the endowment of a few professions. And to make some learn the better, it would do very well that none should come thither sent by their parents, as to a trade to get their living by, but that it should be a place for such ingenuous men, as being free to dispose of their own time, love truth for itself. In the mean time divinity may go on in Oxford and Cambridge to furnish the pulpit with men to cry down the civil power, if they continue to do as they did. If I had, I say, made such a motion in my *Leviathan*, though it would have offended the divines, yet it had been no injury. But it is an injury, you will say, to deny in general the utility of the ancient schools, and to deny that we have received from them our

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geometry. True, if I had not spoken distinctly of the schools of philosophy, and said expressly, that the geometricians passed not then under the name of philosophers; and that in the school of Plato (the best of the ancient philosophers) none were received that were not already in some measure geometricians. Euclid taught geometry; but I never heard of a sect of philosophers called Euclidians, or Alexandrians, or ranged with any of the other sects, as Peripatetics, Stoics, Academics, Epicureans, Pyrrhonians, &c. But what is this to the Universities of Christendom? Or why are we beholden for geometry to our universities, more than to Gresham College, or to private men in London, Paris, and other places, which never taught or learned it in a public school? For even those men that living in our Universities have most advanced the mathematics, attained their knowledge by other means than that of public lectures, where few auditors, and those of unequal proficiency, cannot make benefit by one and the same lesson. And the true use of public professors, especially in the mathematics, being to resolve the doubts, and problems, as far as they can, of such as come unto them with desire to be informed.

That the Universities now are not regulated by the Pope, but by the civil power, is true, and well. But where say I the contrary? And thus much for the first injury.

Another, you say, is this, that in my *Leviathan*, 70, I say: "*The principal schools were or-*
d for the three professions of Roman religion,
n law, and the art of medicine." Thirdly,
say: "*Philosophy had no otherwise place*

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there than as a hand-maid to Roman religion." Fourthly: "*Since the authority of Aristotle was received there, that study is not properly philosophy, but Aristotelity.*" Fifthly: "*That for geometry, till of late times it had no place there at all.*" As for the second, it is too evident to be denied; the fellowships having been all ordained for those professions; and (saving the change of religion) being so yet. Nor hath this any reflection upon the Universities, either as they now are, or as they then were, seeing it was not in their own power to endow themselves, or to receive other laws and discipline than their founder and the state was pleased to ordain. For the third, it is also evident. For all men know that none but the Roman religion had any stipend or preferment in any university, where that religion was established? No, nor for a great while, in their commonwealths; but were everywhere persecuted as heretics. But you will say, the words of my *Leviathan* are not, philosophy "*had no place*," but "*hath no place.*" Are you not ashamed to lay to my charge a mistake of the word *hath* for *had?* which was either a mistake of the printer, or if it were so in the copy, it could be no other than the mistake of a letter in the writing, unless you think you can make men believe that after fifty years being acquainted with what was publicly professed and practised in Oxford and Cambridge, I knew not what religion they were of. This taking of advantage from the mistake of a word, or of a letter, I find also in the *Elenchus*, where for *prætendit se scire*, there is *prætendit scire*, which you the geometrician sufficiently mumble, mistaking it I

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think for an anglicism, not for a fault of the impression.

To the fourth, you pretend, that men are not now so tied to Aristotle as not to *enjoy a liberty of philosophising, though it were otherwise when I was conversant in Magdalen Hall.* Was it so then? Then am I absolved, unless you can show some public act of the university made since that time to alter it. For it is not enough to name some few particular ingenuous men that usurp that liberty in their private discourses, or, with connivance, in their public disputations. And your doctrine, that even here you avow, of *abstracted essences, immaterial substances,* and of *nunc-stans;* and your improper language in using the word (not as mine, for I have it nowhere) *successive eternity;* as also your doctrine of *condensation,* and your arguing from natural reason the incomprehensible mysteries of religion, and your malicious writing, are very shrewd signs that you yourselves are none of those which you say do *freely philosophise;* but that both your philosophy and your language are under the servitude, not of the Roman religion, but of the ambition of some other doctors, that seek, as the Roman clergy did, to draw all human learning to the upholding of their power ecclesiastical. Hitherto therefore there is no injury done to the universities. For the fifth, you grant it, namely, "*that till of late there was no allowance for the teaching of geometry.*" But lest ou should be thought to grant me anything, you y, you the astronomer, "*geometry hath now so ch place in the universities, that when Mr. obbes shall have published his philosophical and*

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geometrical pieces, you assure yourself you shall be able to find a greater number in the university who will understand as much, or more, of them than he desired they should," &c. But though this be true of the *now*, yet it maketh nothing against my *then*. I know well enough that Sir Henry Savile's lectures were founded and endowed since. Did I deny *then* that there were in Oxford many good geometricians? But I deny *now*, that either of you is of the number. For my philosophical and geometrical pieces are published, and you have understood only so much in them, as all men will easily see by your objections to them, and by your own published geometry, that neither of you understand anything either in philosophy or in geometry. And yet you would have those books of yours to stand for an argument, and to be an index of the philosophy and geometry to be found in the universities. Which is a greater injury and disgrace to them, than any words of mine, though interpreted by yourselves.

Your last and greatest accusation, or rather railing (for an accusation should contain, whether true or false, some particular fact, or certain words, out of which it might seem at least to be inferred), is, that I am an enemy to religion. Your words are: "*It is said that Mr. Hobbes is no otherwise an enemy to the Roman religion, saving only as it hath the name of religion.*" This is said by Vindex. You, the geometrician, in your epistle dedicatory, say thus: "*With what pride and imperiousness he tramples on all things both human and divine, uttering fearful and horrible words of God,* (peace), *of sin, of the holy Scripture, of all incor-*

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poreal substances in general, of the immortal soul of man, and of the rest of the weighty points of religion (down), *it is not so much to be doubted as lamented.*" And at the end of your objections to the eighteenth chapter, "*Perhaps you take the whole history of the fall of Adam for a fable, which is no wonder, when you say the rules of honouring and worshipping of God are to be taken from the laws.*" Down, I say; you bark now at the supreme legislative power. Therefore it is not I, but the laws which must rate you off. But do not many other men, as well as you, read my *Leviathan*, and my other books? And yet they all find not such enmity in them against religion. Take heed of calling them all atheists that have read and approved my *Leviathan*. Do you think I can be an atheist and not know it? Or knowing it, durst have offered my atheism to the press? Or do you think him an atheist, or a contemner of the Holy Scripture, that sayeth nothing of the Deity but what he proveth by the Scripture? You that take so heinously that I would have the rules of God's worship in a Christian commonwealth taken from the laws, tell me, from whom you would have them taken? From yourselves? Why so, more than from me? From the bishops? Right, if the supreme power of the commonwealth will have it so; if not, why from them rather than from me? From a consistory of presbyters by themselves, or joined with lay-elders, whom they may sway as they please? Good, if the supreme ernor of the commonwealth will have it so; ot, why from them, rather than from me, or any man else? They are wiser and learn-

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eder than I. It may be so; but it has not yet appeared. Howsoever, let that be granted. Is there any man so very a fool as to subject himself to the rules of other men in those things which so nearly concern himself, for the title they assume of being wise and learned, unless they also have the sword which must protect them. But it seems you understand the sword as comprehended. If so, do you not then receive the rules of God's worship from the civil power? Yes, doubtless; and you would expect, if your consistory had that sword, that no man should dare to exercise or teach any rules concerning God's worship which were not by you allowed. See therefore how much you have been transported by your malice towards me, to injure the civil power by which you live, If you were not despised, you would in some places and times, where and when the laws are more severely executed, be shipped away for this your madness to America, I would say, to Anticyra. What luck have I, when this, of the laws being the rules of God's public worship, was by me said and applied to the vindication of the Church of England from the power of the Roman clergy, it should be followed with such a storm from the ministers, presbyterian and episcopal, of the Church of England? Again, for those other points, namely, that I approve not of incorporeal bodies, nor of other immortality of the soul, than that which the Scripture calleth eternal life, I do but as the Scripture leads me. To the texts whereof by me alleged, you should either have answered, or else forborne to revile me for the conclusions I derived from them. Lastly, what an absurd question is it to ask me

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whether it be in the power of the magistrate, whether the world be eternal or not? It were fit you knew it is in the power of the supreme magistrate to make a law for the punishment of them that shall pronounce publicly of that question anything contrary to that which the law hath once pronounced. The truth is, you are content that the papal power be cut off, and declaimed against as much as any man will; but the ecclesiastical power, which of late was aimed at by the clergy here, being a part thereof, every violence done to the papal power is sensible to them yet; like that which I have heard say of a man, whose leg being cut off for the prevention of a gangrene that began in his toe, would nevertheless complain of a pain in his toe, when his leg was cut off.

Thus much in my defence; which I believe if you had foreseen, this accusation of yours had been left out. I come now to examine (though it be done in part already) what manners those are which I find everywhere in your writings.

And first, how came it into your minds that a man can be an atheist, I mean an atheist in his conscience? I know that David confesseth of himself, upon sight of the prosperity of the wicked, that his feet had almost slipped, that is, that he had slipped into a short doubtfulness of the Divine Providence. And if anything else can cause a man to slip in the same kind, it is the seeing such as you (who though you write nothing but what
d to each of you by a doctor of divinity)
he greatest of God's commandments,
arity, in every line before his face.
such forgettings of God be somewhat

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more than short doubtings, and sudden transportations incident to human passion, yet I do not for that cause think you atheists and enemies of religion, but only ignorant and imprudent Christians. But how, I say, could you think me an atheist, unless it were because finding your doubts of the Deity more frequent than other men do, you are thereby the apter to fall upon that kind of reproach? Wherein you are like women of poor and evil education when they scold; amongst whom the readiest disgraceful word is whore: why not thief, or any other ill name, but because, when they remember themselves, they think that reproach the likeliest to be true?

Secondly, tell me what crime it was which the Latins called by the name of *scelus?* You think not, unless you be Stoics, that all crimes are equal. *Scelus* was never used but for a crime of greatest mischief, as the taking away of life and honour; and besides, basely acted, as by some clandestine way, or by such a way as might be covered with a lie. But when you insinuate in a writing published that I am an atheist, you make yourselves authors to the multitude, and do all you can to stir them up to attempt upon my life; and if it succeed, then to sneak out of it by leaving the fault on them that are but actors. This is to endeavour great mischief basely, and therefore *scelus.* Again, to deprive a man of the honour he hath merited, is no little wickedness; and this you endeavour to do by publishing falsely that I challenge as my own the inventions of other men. This is therefore *scelus* publicly to tell all the world that I will be angry with all men that do not presently submit to my dictates; to deprive me of the friend-

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ship of all the world; great damage, and a lie, and yours. For to publish any untruth of another man to his disgrace, on hearsay from his enemy, is the same fault as if he published it on his own credit. If I should say I have heard that Dr. Wallis was esteemed at Oxford for a simple fellow, and much inferior to his fellow-professor Dr. Ward (as indeed I have heard, but do not believe it), though this be no great disgrace to Dr. Wallis, yet he would think I did him injury. Therefore public accusation upon hearsay is *scelus*. And whosoever does any of these things does *sceleratè*. But you the professors of the mathematics at Oxford, by the advice of two doctors of divinity have dealt thus with me. Therefore you have done, I say not foolishly, though no wickedness be without folly, but *sceleratè*, ὅπερ ἔδει δεῖξαι.

Thirdly, it is ill manners, in reprehending truth, to send a man in a boasting way to your own errors; as you the professor of geometry have often sent me to your two tractates of the *Angle of Contact* and *Arithmetica Infinitorum*.

Fourthly, it is ill manners, to diminish the just reputation of worthy men after they be dead, as you the professor of geometry have done in the case of Joseph Scaliger.

Fifthly, when I had in my *Leviathan* suffered the clergy of the Church of England to escape, you did imprudently in bringing any of them in again. An Ulysses upon so light an occasion would ave ventured to return again into the cave of hemus.

ly, how ill does such levity and scurrility, both of you have shown so often in your gs, become the gravity and sanctity requisite to the calling of the ministry? They are too many to be repeated. Do but consider, you the geometrician, how unhandsome it is to play upon my name, when both yours and mine are plebeian names; though from Willis by Wallis, you go from yours in Wallisius. The jest of using at every word *mi Hobbi*, is lost to them beyond sea. But this is not so ill as some of the rest. I will write but one of them, as it is in the fourth page of your *Elenchus*: "*Whence it appears that your Empusa was of the number of those fairies which you call n English hob-goblins. The word is made of* ν and πȣς; *and thence comes the children's play called the play of Empusa, Anglicè* (hitherto in Latin all but *hob-goblins*, then follows in English) *fox, fox, come out of your hole* (then in Latin again), *in which the boy that is called the fox, holds up one foot, and jumps with the other, which n English is to hop*." When a stranger shall read this, and hoping to find therein some witty conceit, shall with much ado have gotten it interpreted and explained to him, what will he think of our doctors of divinity at Oxford, that will take so much pains as to go out of the language they set forth in, for so ridiculous a purpose? You will say it is a pretty *paranomasia*. How you call t there I know not, but it is commonly called here a *clinch;* and such a one as is too insipid for a boy of twelve years old, and very unfit for the sanctity of a minister, and gravity of a doctor of divinity. But I pray you tell me where it was you read the word *empusa* for the boy's play you speak of, or for any other play amongst the Greeks? In this (as you have done throughout all your other writings) you presume too much upon your first

LESSON VI.

Of manners.

cogitations. There be a hundred other scoffing passages, and ill-favoured attributes given me in both your writings, which the reader will observe without my pointing to them, as easily as you would have him; and which perhaps some young students, finding them full of gall, will mistake for salt. Therefore to disabuse those young men, and to the end they may not admire such kind of wit, I have here and there been a little sharper with you than else I would have been. If you think I did not spare you, but that I had not wit enough to give you as scornful names as you give me, are you content I should try? Yes (you the geometrician will say) give me what names you please, so you call me not *Arithmetica Infinitorum.* I will not. Nor *Angle of Contact;* nor *Arch Spiral;* nor *Quotient.* I will not. But I here dismiss you both together. So go your ways, you *Uncivil Ecclesiastics, Inhuman Divines, Dedoctors of morality, Unasinous Colleagues, Egregious pair of Issachars, most wretched Vindices and Indices Academiarum;* and remember Vespasian's law, that it is uncivil to give ill language first, but civil and lawful to return it. But much more remember the law of God, to obey your sovereigns in all things; and not only not to derogate from them, but also to pray for them, and as far as you can to maintain their authority, and therein your own protection. And, do you hear? take heed of
king your mind so clearly in answering my
han, as I have done in writing it. You
do best not to meddle with it at all, because
ndertaken, and in part published already,
be better performed, from term to term,
Christopher Pike.

ΣΤΙΓΜΑΙ

Αγεωμετρίας, Αγροικίας, Αντίπολιτείας, Αμαθείας,

OR

MARKS

OF THE

ABSURD GEOMETRY, RURAL LANGUAGE, SCOTTISH
CHURCH POLITICS, AND BARBARISMS

OF

JOHN WALLIS,

PROFESSOR OF GEOMETRY AND DOCTOR OF DIVINITY.

BY

THOMAS HOBBES,

OF MALMESBURY.

TO THE RIGHT HONOURABLE

HENRY, LORD PIERREPONT,

VISCOUNT NEWARK, EARL OF KINGSTON, AND
MARQUIS OF DORCHESTER.

MY MOST NOBLE LORD,

I DID not intend to trouble your Lordship twice with this contention between me and Dr. Wallis. But your Lordship sees how I am constrained to it; which, whatsoever reply the Doctor makes, I shall be constrained to no more. That which I have now said of his Geometry, Manners, Divinity, and Grammar, altogether is not much, though enough. As for that which I here have written concerning his Geometry, which you will look for first, is so clear, that not only your Lordship, and such as have proceeded far in that science, but also any man else that doth but know how to add and subtract proportions, (which is taught at the twenty-third proposition of the sixth of Euclid), may see the Doctor is in the wrong. That which I say of his ill language and politics is yet shorter. The rest, which concerneth grammar, is almost all another man's, but so full of learning of that kind, as no man that taketh delight in knowing the proprieties of the Greek and Latin tongues, will think his time ill bestowed in the reading it. I give the Doctor no more ill words, but am returned from his manners to my own. Your Lordship may perhaps say, my compliment in my title-page is somewhat coarse; and it is true. But, my Lord, it is since the writing of the title-page, that I am returned from the Doctor's manners to my own; which are such as I hope you will not be ashamed to own me, my Lord, for one of

Your Lordship's most humble
and obedient servants,

THOMAS HOBBES.

TO

DOCTOR WALLIS,

IN ANSWER TO HIS

SCHOOL DISCIPLINE.

SIR,

Marks of Dr. Wallis's absurd Geometry, &c.

WHEN unprovoked you addressed unto me, in your *Elenchus*, your harsh compliment with great security, wantonly to show your wit, I confess you made me angry, and willing to put you into a better way of considering your own forces, and to move you a little as you had moved me, which I perceive my lessons to you have in some measure done; but here you shall see how easily I can bear your reproaches, now they proceed from anger, and how calmly I can argue with you about your geometry and other parts of learning.

I shall in the first part confer with you about your *Arithmetica Infinitorum*, and afterwards compare our manner of elocution; then your politics; and last of all your grammar and critics. Your spiral line is condemned by him whose authority you use to prove me a plagiary, (that is, a man that stealeth other men's inventions, and arrogates them to himself), whether it be Roberval or not that writ that paper, I am not certain. But I think I shall be shortly; but whosoever it be, his authority will serve no less to

Marks of Dr. Wallis's absurd Geometry, &c.

show that your doctrine of the spiral line, from the fifth to the eighteenth proposition of your *Arithmetica Infinitorum*, is all false; and that the principal fault therein (if all faults be not principal in geometry, when they proceed from ignorance of the science) is the same that I objected to you in my *Lessons*. And for the author of that paper, when I am certain who it is, it will be then time enough to vindicate myself concerning that name of plagiary. And whereas he challenges the invention of your method delivered in your *Arithmetica Infinitorum*, to have been his before it was yours, I shall, I think, by and by say that which shall make him ashamed to own it; and those that writ those encomiastic epistles to you ashamed of the honour they meant to you. I pass therefore to the nineteenth proposition, which in Latin is this: your geometry!

"*Si proponatur series quantitatum in duplicata ratione arithmetice proportionalium (sive juxta seriem numerorum quadraticorum) continue crescentium, a puncto vel* 0 *inchoatarum, (puta ut* 0. 1. 4. 9. 16. *etc.), propositum sit, inquirere quam habeat illa rationem ad seriem totidem maximæ æqualium.*

"*Fiat investigatio per modum inductionis ut (in prop.* 1)

Eritque,

$$\frac{0+1=1}{1+1=2}=\frac{1}{3}+\frac{1}{6}$$

$$\frac{0+1+4=5}{4+4+4=12}=\frac{1}{3}+\frac{1}{12}$$

$$\frac{+1+4+9=14}{9+9+9=36}=\frac{1}{3}+\frac{1}{18}$$ *et sic deinceps.*

proveniens est ubique major quam

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subtripla seu $\frac{1}{3}$*; excessus autem perpetuo decrescit prout numerus terminorum augetur (puta* $\frac{1}{6}$ $\frac{1}{12}$ $\frac{1}{18}$ $\frac{1}{24}$ *etc.) aucto nimirum fractionis denominatore sive consequente rationis in singulis locis numero senario (ut patet) ut sit rationis provenientis excessus supra subtriplam, ea quam habet unitas ad sextuplum numeri terminorum post* 0*; adeoque*."

That is, if there be propounded a row of quantities in duplicate proportion of the quantities arithmetically proportional (or proceeding in the order of the square numbers) continually increasing; and beginning at a point or 0; let it be propounded to find what proportion the row hath; to as many quantities equal to the greatest;

Let it be sought by induction (as in the first proposition).

The proportion arising is everywhere greater than subtriple, or $\frac{1}{3}$, and the excess perpetually decreaseth as the number of terms is augmented, as here, $\frac{1}{6}$ $\frac{1}{12}$ $\frac{1}{18}$ $\frac{1}{24}$ $\frac{1}{30}$, &c. the denominator of the fraction being in every place augmented by the number six, as is manifest; so that the excess of the rising proportion above subtriple is the same which unity hath to six times the number of terms after 0; and so.

Sir, in these your characters I understand by the cross + that the quantities on each side of it are to be added together and make one aggregate; and I understand by the two parallel lines = that the quantities between which they are placed are one to another equal; this is your meaning, or you should have told us what you meant else; I understand also, that in the first row $0 + 1$ is equal to 1,

Marks of Dr. Wallis's absurd Geometry, &c.

and 1+1 equal to 2; and that in the second row 0+1+4 is equal to 5; and 4+4+4 equal to 12; but (which you are too apt to grant) I understand your symbols no further; but must confer with yourself about the rest.

And first I ask you (because fractions are commonly written in that manner) whether in the uppermost row (which is $\frac{0+1=1}{1+1=2}=\frac{1}{3}+\frac{1}{6})\frac{0}{1}$ be a fraction, $\frac{1}{1}$ be a fraction, $\frac{1}{2}$ be a fraction, that is to say, a part of an unit, and if you will, for the cypher's sake, whether $\frac{0}{1}$, be an infinitely little part of 1; and whether $\frac{1}{1}$, or 1 divided by 1 signify an unity? if that be your meaning, then the fraction $\frac{0}{1}$ added to the fraction $\frac{1}{1}$ is equal to the fraction $\frac{1}{2}$: But the fraction $\frac{0}{1}$ is equal to 0; therefore the fraction $\frac{0}{1}+\frac{1}{1}$ is equal to the fraction $\frac{1}{1}$; and $\frac{1}{1}$ equal to $\frac{1}{2}$ which you will confess to be an absurd conclusion, and cannot own that meaning.

I ask you therefore again, if by $\frac{0}{1}$ you mean the proportion of 0 to 1; and consequently by $\frac{1}{1}$ the proportion of 1 to 1, and by $\frac{1}{2}$ the proportion of 1 to 2: if so, then it will follow, that if the proportions of 0 to 1 and of 1 to 1 be compounded by addition, the proportion arising will be the proportion of 1 to 2. But the proportion of 0 to 1 is infinitely little, that is, none. Therefore the proposition arising by composition will be that of 1 to 1, and equal (because of the symbol =) to the proportion of 1 to 2, and so 1=2. This also is so absurd that I dare say that you will not own it.

T[illegible] be another meaning yet: perhaps [illegible] uppermost quantity 0+1 is equal

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to the uppermost quantity 1; and the lowermost quantity $1+1$ equal to the lowermost quantity 2: which is true. But how then in this equation $\frac{1}{2}=\frac{1}{3}+\frac{1}{6}$? Is the uppermost quantity 1 equal to the uppermost quantity $1+1$; or the lowermost quantity 2 equal to the lowermost quantity $3+6$? Therefore neither can this be your meaning. Unless you make your symbols more significant, you must not blame me for want of understanding them.

Let us now try what better success we shall have where the places are three, as here:

$$\frac{0+1+4=5}{4+4+4=12}=\frac{5}{12}=\frac{1}{3}+\frac{1}{12}:$$

If your symbols be fractions, the compound of them by addition is $\frac{5}{4}$, for $0\frac{1}{4}$ and $\frac{4}{4}$ make $\frac{5}{4}$; and consequently (because of the symbol $=$) $\frac{5}{4}$ equal to $\frac{5}{12}$, which is not to be allowed, and therefore that was not your meaning. If you meant that the proportions of 0 to 4 and of 1 to 4 and of 4 to 4 compounded, is equal to the proportion of 5 to 12, you will fall again into no less an inconvenience. For the proportion arising out of that composition will be the proportion of 1 to 4. For the proportion of 0 to 4 is infinitely little. Then to compound the other two, set them in this order 1. 4. 4. and you have a proportion compounded of 1 to 4 and of 4 to 4, namely, the proportion of the first to the last, which is of 1 to 4, which must be equal, by this your meaning, to the proportion of 5 to 12, and consequently as 5 to 12, so is 1 to 4, which you must not own. Lastly, if you mean that the uppermost quantities to the uppermost, and the lowermost to the lowermost in the first

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equation are equal, it is granted, but then again in the second equation it is false. It concerns your fame in the mathematics to look about how to justify these equations which are the premises to your conclusion following, namely, that the proportion arising is every where greater than subtriple, or a third ; and that the excess (that is, the excess above subtriple) perpetually decreaseth as the number of terms is augmented, as here $\frac{1}{6}\,\frac{1}{12}\,\frac{1}{18}\,\frac{1}{24}\,\frac{1}{30}$, &c. which I will show you plainly is false.

But first I wonder why you were so angry with me for saying you made proportion to consist in the quotient, as to tell me it was abominably false, and to justify it, cite your own words *penes quotientem ;* do not you say here, the proportion is everywhere greater than subtriple, or $\frac{1}{3}$? And is not $\frac{1}{3}$ the quotient of 1 divided by 3 ? You cannot say in this place that *penes* is understood : for if it were expressed you would not be able to proceed.

But I return to your conclusion, that the excess of the proportion of the increasing quantities above the third part of so many times the greatest, decreaseth, as $\frac{1}{6}\,\frac{1}{12}\,\frac{1}{18}\,\frac{1}{24}\,\frac{1}{30}$, &c. For by this account in this row $\frac{0+1}{1+1}=\frac{1}{2}$ where the quantity above exceeds the third part of the quantities below by $\frac{1}{3}$, you make $\frac{1}{3}$ equal to $\frac{1}{6}$, which you do not mean. It may be said your meaning is, that the proportion of 1 to the subtriple of 2 which is $\frac{2}{3}$, exceedeth what ? I cannot imagine what, nor proceed further where the terms be but two. Let us therefore take the second row, that is, $\frac{0+1+4=5}{4+4+4=12}$. The

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sum above is 5, the sum below is 12, the third part whereof is 4; if you mean, that the proportion of 5 to 4 exceeds the proportion of 4 to 12 (which is subtriple) by $\frac{1}{12}$, you are out again. For 5 exceeds 4 by unity, which is $\frac{12}{12}$. I do not think you will own such an equation as $\frac{12}{12}=\frac{1}{12}$. Therefore I believe you mean (and your next proposition assures me of it), that the proportion of 5 to 4 exceeds subtriple proportion by the proportion of 1 to 12; if you do so, you are yet deceived.

For if the proportion of 5 to 4 exceeds subtriple proportion by the proportion of 1 to 12, then subtriple proportion, that is, of 4 to 12 added to the proportion of 1 to 12 must make the proportion of 5 to 4. But if you look on these quantities, 4, 12, 144, you will see, and must not dissemble, that the proportion of 4 to 12 is subtriple, and the proportion of 12 to 144 is the same with that of 1 to 12. Therefore by your assertion it must be as 5 to 4 so 4 to 144, which you must not own.

And yet this is manifestly your meaning, as appeareth in these words: "*Ut sit rationis provenientis excessus supra subtriplam ea quam habet unitas ad sextuplum numeri terminorum post* 0, *adeoque*," which cannot be rendered in English, nor need to be. For you express yourself in the twentieth proposition very clearly; I noted it only that you may be more merciful hereafter to the stumblings of a hasty pen. For *excessus ea quam* does not well, nor is to be well excused by *subauditur ratio*. Your twentieth proposition is this:

"*Si proponatur series quantitatum in duplicata ratione arithmetice proportionalium (sive juxta*

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seriem numerorum quadraticorum) continue crescentium, a puncto vel 0 *inchoatarum, ratio quam habet illa ad seriem totidem maximæ æqualium subtriplam superabit; eritque excessus ea ratio quam habet unitas ad sextuplum numeri terminorum post* 0, *sive quam habet radix quadratica termini primi post* 0 *ad sextuplum radicis quadraticæ termini maximi.*"

That is, if there be propounded a row of quantities in duplicate proportion of arithmetically-proportionals (or according to the row of square numbers) continually increasing, and beginning with a point or 0. The proportion of that row to a row of so many equals to the greatest, shall be greater than subtriple proportion, and the excess shall be that proportion which unity hath to the sextuple of the number of terms after 0, or the same which the square root of the first number after 0, hath to the sextuple of the square root of the greatest.

For proof whereof you have no more here than *patet ex præcedentibus;* and no more before but *adeoque*. You do not well to pass over such curious propositions so slightly; none of the ancients did so, nor, that I remember, any man before yourself. The proposition is false, as you shall presently see.

Take, for example, any one of your rows: as $\frac{0+1+4}{4+4+4}$. By this proportion of yours 1+4, which makes 5, is to 12 in more than subtriple proportion; by the proportion of 1 to the sextuple of 2 which is 12. Put in order these three quantities 5, 4, 12, and you must see the proportion of 5 to 12 is greater than the proportion of 4 to 12, that is, subtriple proportion, by the proportion of 5 to 4.

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But by your account the proportion of 5 to 4 is greater than that of 4 to 12 by the proportion of 1 to 12. Therefore, as 5 to 4 so is 1 to 12, which is a very strange paradox.

After this you bring in this consectary: "*Cum autem crescente numero terminorum excessus ille supra rationem subtriplam continue minuatur, ut tandem quovis assignabili minor evadat* (*ut patet*) *si in infinitum producatur, prorsus evaniturus est. Adeoque.*"

That is, seeing as the number of terms increaseth, that excess above subtriple proportion continually decreaseth, so as at length it becomes less than any assignable (as is manifest) if it be produced infinitely, it shall utterly vanish, and so. And so what?

Sir, this consequence of yours is false. For two quantities being given, and the excess of the greater above the less, that excess may continually be decreased, and yet never quite vanish. Suppose any two unequal quantities differing by more than an unit, as 3 and 6, the excess 3, let 3 be diminished, first by an unit, and the excess will be 2, and the quantities will be 3 and 5; 5 is greater than 4, the excess 1. Again, let 1 be diminished and made $\frac{1}{2}$, the excess $\frac{1}{2}$ and the quantities 3 and $4\frac{1}{2}$, $4\frac{1}{2}$ is yet greater than 4. Again diminish the excess to $\frac{1}{4}$, the quantities will be 3 and $4\frac{1}{4}$, yet still $4\frac{1}{4}$ is greater than 4. In the same manner you may proceed to $\frac{1}{8}$ $\frac{1}{16}$ $\frac{1}{32}$, &c. infinitely; and yet you shall never come within an unit (though your unit stand for 100 miles) of the lesser quantity propounded 3, if that 3 stands for 300 miles. The excesses above subtriple proportion do not decrease

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in the manner you say it does, but in the manner which I now shall show you.

In the first row $\frac{0+1}{1+1}$ a third of the quantities below is $\frac{2}{3}$, set in order these three quantities $1\ \frac{2}{9}\ \frac{2}{3}$. The first is 1, equal to the sum above, the last is $\frac{2}{3}$, equal to the subtriple of the sum below. The middlemost is $\frac{2}{9}$ subtriple to the last quantity $\frac{2}{3}$. The excess of the proportion of 1 to $\frac{2}{3}$ above the subtriple proportion of $\frac{2}{9}$ to $\frac{2}{3}$ is the proportion of 1 to $\frac{2}{9}$, that is of 9 to 2, that is, of 18 to 4.

Secondly, in the second row, which is $\frac{0+1+4}{4+4+4}$, a third of the sum below is 4, the sum above is 5. Set in order these quantities, 1, 5, 4, 12. There the proportion of 15 to 12 is the proportion of 5 to 4. The proportion of 4 to 12 is subtriple; the excess is the proportion of 15 to 4, which is less than the proportion of 18 to 4, as it ought to be; but not less by the proportion of $\frac{1}{6}$ to $\frac{1}{12}$ as you would have it.

Thirdly, in the third row, which is $\frac{0+1+4+9}{9+9+9+9}$. A third of the sum below is 12, the sum above is 14. Set in order these quantities, 42, 4, 12. There the proportion of 42 to 12 is the same with that of 14 to 4. And the proportion of 4 to 12 subtriple, less than the former excess of 15 to 4. And so it goes on decreasing all the way in this manner, 18 to 4, 15 to 4, 14 to 4, &c. which differs very much from your 1 to 6, 1 to 12, 1 to 18, &c. nd the cause of your mistake is this: you call e twelfth part of twelve $\frac{1}{12}$, and the eighteenth rt of thirty-six you call $\frac{1}{18}$, and so of the rest. t what need of all those equations in symbols, show that the proportion decreases; is there

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any man can doubt, but that the proportion of 1 to 2 is greater than that of 5 to 12, or that of 5 to 12 greater than that of 14 to 36, and so on continually forwards; or could you have fallen into this error, unless you had taken, as you have done in very many places of your *Elenchus*, the fractions $\frac{1}{6}$ and $\frac{1}{12}$, &c. which are the quotients of 1 divided by 6 and 12, for the very proportions of 1 to 6 and 1 to 12. But notwithstanding the excess of the proportions of the increasing quantities, to subtriple proportion decrease, still, as the number of terms increaseth, and that what proportions soever I shall assign, the decrement will in time (in time, I say, without proceeding *in infinitum*) produce a less, yet it does not follow that the row of increasing quantities shall ever be equal to the third part of the row of so many equals to the last or greatest. For it is not, I hope, a paradox to you, that in two rows of quantities the proportion of the excesses may decrease, and yet the excesses themselves increase, and do perpetually.

For in the second and third rows, which are $\frac{0+1+4=5}{4+4+4=12}$ and $\frac{0+1+4+9=14}{9+9+9+9=36}$ 5 exceeds the third part of 12 by a quarter of the square of 4, and 14 exceeds the third part of 36 by 2 quarters of the square of 4, and proceeding on, the sum of the increasing quantities where the terms are 5 (which sum is 30) exceedeth the third part of those below, (those below are 80, and their third part $26\frac{2}{3}$) by 3 quarters and $\frac{1}{2}$ a quarter of the square of 4, and when the terms are 6, the quantities above will exceed the third part of them below by 5 quarters of the square of 4. Would you have men believe,

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that the further they go, the excess of the increasing quantities above the third part of those below shall be so much the less? And yet the proportions of those above, to the thirds of those below, shall decrease eternally; and therefore your twenty-first proposition is false, namely this:

"*Si proponatur series infinita quantitatum in duplicata ratione arithmetice proportionalium* (*sive juxta seriem numerorum quadraticorum*), *continue crescentium a puncto sive* 0 *inchoatarum; erit illa ad seriem totidem maximæ æqualium, ut* 1 *ad* 3."

That is, if an infinite row of quantities be propounded in duplicate proportion of arithmetically-proportionals (or according to the row of quadratic numbers), continually increasing and beginning from a point or 0; that row shall be to the row of as many equals to the greatest, as 1 to 3. This is false, *ut patet ex præcedentibus;* and, consequently, all that you say in proof of the proportion of your *parabola* to a *parallelogram*, or of the *spiral* (the true *spiral*) to a *circle* is in vain.

But your spiral puts me in mind of what you have under-written to the diagram of your proposition 5. *The spiral, in both figures, was to be continued whole to the middle, but, by the carelessness of the graver, it is in one figure* manca, *in the other* intercisa.

Truly, Sir, you will hardly make your reader believe that a graver could commit those faults without the help of your own copy, nor that it had been in your copy, if you had known how to describe a spiral line then as now. This I had not said, though truth, but that you are pleased to say,

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though not truth, that I attributed to the printer some faults of mine.

I come now to the thirty-ninth proposition, which is this:

"*Si proponatur series quantitatum in triplicata ratione arithmetice proportionalium* (*sive juxta seriem numerorum cubicorum*), *continue crescentium a puncto sive* 0 *inchoatarum* (*puta ut* 0, 1, 8, 27, *etc.*), *propositum sit inquirere quam habeat series illa rationem ad seriem totidem maximæ æqualium*:

"*Fiat investigatio per modum inductionis* (*ut in prop.* 1, *et prop.* 19):

Eritque

$$\frac{0+1=1}{1+1=2}=\frac{2}{4}=\frac{1}{4}+\frac{1}{4}$$

$$\frac{0+1+8=9}{8+8+8=24}=\frac{3}{8}=\frac{1}{4}+\frac{1}{8}$$

$$\frac{0+1+8+27=36}{27+27+27+27=108}=\frac{4}{12}=\frac{1}{4}+\frac{1}{12}$$

Et sic deinceps.

"*Ratio proveniens est ubique major quam subquadrupla, sive* $\frac{1}{4}$. *Excessus autem perpetuo decrescit, pro ut numerus terminorum augetur, puta* $\frac{1}{4}\frac{1}{8}\frac{1}{12}\frac{1}{16}$ *etc. Aucto nimirum fractionis denominatore sive consequente rationis in singulis locis numero quaternatio, ut patet, ut sit rationis provenientis excessus supra subquadruplam ea quam habet unitas ad quadruplum numeri terminorum post* 0 *adeoque.*"

That is, if a row of quantities be propounded in triplicate proportion of arithmetically proportionals (or according to the row of cubic numbers), continually increasing, and beginning from a point or 0, as 0, 1, 8, 27, 64, &c., let it be propounded to

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inquire, what proportion that row hath to a row of as many equals to the greatest.

Be it sought by way of induction, as in proposition 1 and 19.

The proposition arising is everywhere greater than subquadruple, or $\frac{1}{4}$, and the excess perpetually decreaseth as the number of terms increaseth, as $\frac{1}{4}$ $\frac{1}{8}$ $\frac{1}{12}$ $\frac{1}{16}$ $\frac{1}{20}$ &c. The denominator of the fraction, or consequent of the proportion, being in every place augmented by the number 4, as is manifest, so that the excess of the arising proportion above subquadruple is the same with that which an unit hath to the quadruple of the number of the terms after 0, and so. Here are just the same faults which are in proposition 19.

For, if $\frac{0}{1}$ be a fraction, and $\frac{1}{1}$ be a fraction, and $\frac{1}{2}$ be another fraction, then this equation $\frac{0+1=1}{1+1=2}$ is false. For this fraction $\frac{0}{1}$ is equal to 0; and, therefore, we have $\frac{1}{1}=\frac{1}{2}$, that is, the whole equal to half. But perhaps you do not mean them fractions, but proportions; and, consequently, that the proportion of 0 to 1, and of 1 to 1, compounded by addition (I say by addition, not that I, but that you think there is a composition of proportions by multiplication, which I shall show you anon is false), must be equal to the proportion of 1 to 2, which cannot be. For the proportion of 0 to 1 is infinitely little, that is, none at all; and, consequently, the proportion of 1 to 1 is equal to the proportion of 1 to 2, which is again absurd.

e is no doubt but the whole number of 0 + 1
al to 1, and the whole number of 1 + 1 equal
But, reckoning them as you do, not for

whole numbers, but for fractions or proportions, the equations are false.

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Again, your second equation, $\frac{2}{4}=\frac{1}{4}+\frac{1}{4}$, though meant of fractions, that is, of quotients, it be true, and serve nothing to your purpose, yet, if it be meant of proportions, it is false. For the proportion of 1 to 4, and of 1 to 4 being compounded, are equal to the proportion of 1 to 16, and so you make the proportion of 2 to 4 equal to the proportion of 1 to 16, where, as it is but subquaduplicate, as you call it, or the quarter of it, as I call it. And, in the same manner, you may demonstrate to yourself the same fault in all the other rows of how many terms soever they consist. Therefore, you may give for lost this thirty-ninth proposition, as well as all the other thirty-eight that went before. As for the conclusion of it, which is, *that the excess of the arising proportion*, &c. They are the words of your fortieth proposition, where you express yourself better, and make your error more easy to be detected.

The proposition is this:

" *Si proponatur series quantitatum in triplicata ratione arithmetice proportionalium* (*sive juxta seriem numerorum cubicorum*) *continue crescentium a puncto vel* 0 *inchoatarum, ratio quam habet illa ad seriem totidem maximæ æqualium subquadruplam superabit; eritque excessus ea ratio quam habet unitas ad quadruplum numeri terminorum post* 0*; sive quam habet radix cubica termini primi post* 0 *ad quadruplum radicis cubicæ termini maximi. Patet ex præcedente.*

" *Quum autem crescente numero terminorum excessus ille supra rationem subquadruplam ita*

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continuo minuatur, ut tandem quolibet assignabili minor evadat, ut patet, si in infinitum procedatur, prorsus evaniturus est, adeoque.

“ *Patet ex propositione præcedente.*

That is, if a row of quantities be propounded in triplicate proportion of arithmetically proportionals (or according to the row of cubic numbers), continually increasing, and beginning at a point or 0; the proportion which that row hath to a row of as many equals to the greatest, is greater than subquadruple proportion; and the excess is that proportion which one unit hath to the quadruple of the number of terms after 0; or, which the cubic root of the first term after 0 hath to the quadruple of the root of the greatest term.

It is manifest by the precedent propositions.

And, seeing the number of terms increasing, that excess above quadruple proportion doth so continually decrease, as that, at length, it becomes less than any proportion that can be assigned, as is manifest, if the proceeding be infinite, it shall quite vanish. And so

This conclusion was annexed to the end of your thirty-ninth proposition, as there proved. What cause you had to make a new proposition of it, without other proof than *patet ex præcedente*, I cannot imagine. But, howsoever, the proposition is false.

For example, set forth any of your rows, as this of fewer terms:

$$\frac{0+1+8+27=36}{27+27+27+27=108}$$

The row above is 36, the fourth part of the row below is 27. The quadruple of the number of terms after 0 is 12. Then, by your account,

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the proportion of 36 to 108 is greater than subquadruple proportion by the proportion of 1 to 12. For trial whereof, set in order these three quantities, 36, 27, 108. The proportion of 36 (the uppermost row) to 108 (the lowermost row) is compounded by addition of the proportions 36 to 27, and 27 to 108. And the proportion of 36 to 108, exceedeth the proportion of 27 to 108, by the proportion of 36 to 27. But the proportion of 27 to 108 is subquadruple proportion. Therefore, the proportion of 36 to 108 exceedeth subquadruple proportion, by the proportion of 36 to 27. And, by your account, by the proportion of 1 to 12; and, consequently, as 36 to 27, so is 1 to 12. Did you think such demonstrations as these should always pass?

Then, for your inference from the decrease of the proportions of the excess, to the vanishing of the excess itself, I have already showed it to be false; and by consequence that your next proposition, namely, the fortieth, is also false.

The proposition is this:

"*Si proponatur series infinita quantitatum in triplicata ratione arithmetice proportionalium (sive juxta seriem numerorum cubicorum), continue crescentium a puncto sive* 0 *inchoatarum, erit illa ad seriem totidem maximæ æqualium, ut* 1 *ad* 4, *patet ex præcedente.*"

That is, if there be propounded an infinite row of quantities in triplicate proportion of arithmetically proportionals (or according to the row of cubic numbers), continually increasing, and beginning at a point or 0; it shall be to the row of as many equals to the greatest as 1 to 4. Manifest out of the precedent proposition.

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Even as manifest as that 36, 27, 1, 12, are proportionals. Seeing, therefore, your doctrine of the spiral lines and the spaces is given by yourself for lost, and a vain attempt, your first forty-one propositions are undemonstrated, and the grounds of your demonstrations all false. The cause whereof is partly your taking quotient for proportion, and a point for 0, as you do in the first, sixteenth, and fortieth propositions, and in other places where you say, *beginning at a point or* 0, though now you deny you ever said either. There be very many places in your *Elenchus*, where you say both; and have no excuse for it, but that, in one of the places, you say the proportion is *penes quotientem*, which is to the same or no sense.

Your forty-second proposition is grounded on the fortieth; and therefore, though true, and demonstrated by others, is not demonstrated by you.

Your forty-third is this:

"*Pari methodo invenietur ratio seriei infinitæ quantitatum arithmetice proportionalium in ratione quadruplicata, quintuplicata, sextuplicata, etc., arithmetice proportionalium a puncto seu* 0 *inchoatarum, ad seriem totidem maximæ æqualium. Nempe in quadruplicata erit, ut* 1 *ad* 5; *in quintuplicata, ut* 1 *ad* 6; *in sextuplicata, ut* 1 *ad* 7. *Et sic deinceps.*"

That is, by the same method will be found, the proportion of an infinite row of arithmetically proportionals, in proportion quadruplicate, quintuplicate, sextuplicate, &c., of arithmetically proportionals, beginning at a point or 0, to the row of as many equals to the greatest; namely, in quadru-

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plicate, it shall be as 1 to 5; in quintuplicate, as 1 to 6; in sextuplicate, as 1 to 7; and so forth.

But by the same method that I have demonstrated, that the propositions 19, 20, 21, 39, 40, and 41, are false: any man else, that will examine the forty-third may find it false also. And, because all the rest of the propositions of your *Arithmetica Infinitorum* depend on these, they may safely conclude, that there is nothing demonstrated in all that book, though it consist of 194 propositions. The proportions of your parabolocides to their parallelograms are true, but the demonstrations false, and infer the contrary. Nor were they ever demonstrated (at least the demonstrations are not extant) but by me; nor can they be demonstrated, but upon the same grounds, concerning the nature of proportion, which I have clearly laid, and you not understood. For, if you had, you could never have fallen into so gross an error as is this your book of *Arithmetica Infinitorum*, or that of the angle of contact. You may see by this, that your symbolic method is not only not at all inventive of new theorems, but also dangerous in expressing the old. If the best masters of symbolics think for all this you are in the right, let them declare it. I know how far the analysis by the powers of the lines extendeth, as well as the best of your half-learnt epistlers, that approve so easily of such analogisms as those, 5, 4, 1, 12, and 36, 27, 1, 12, &c.

It is well for you that they who have the disposing of the professors' places take not upon them to be judges of geometry. For, if they did, seeing you confess you have read these doctrines in your

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school, you had been in danger of being put out of your place.

When the author of the paper wherein I am called Plagiary, and wherein the honour is taken from you of being the first inventor of these fine theorems, shall read this that I have here written, he will look to get no credit by it; especially if it be Roberval, which methinks it should not be. For he understands what proportion is, better than to make 5 to 4 the same with 1 to 12. Or to make, again, the proportion of 36 to 27 the same with that of 1 to 12; and innumerable *disproportionalites* that may be inferred from the grounds you go on. But if it be Roberval indeed, that snatches this invention from you, when he shall see this burning coal hanging at it, he will let it fall again, for fear of spoiling his reputation.

But what shall I answer to the authority of the three great mathematicians that sent you those encomiastic letters. For the first, whom you say I use to praise, I shall take better heed hereafter of praising any man for his learning whilst he is young, further than that he is in a good way. But it seems he was in too ready a way of thinking very well of himself, as you do of yourself. For the muddiness of my brain I must confess it; but, Sir, ought not you to confess the same of yours? No, men of your tenets use not to do so. He wonders, say you, you thought it worth the while to foul your fingers about such a piece. It is well; every an abounds in his own sense. If you and I were be compared by the compliments that are given in private letters, both you and your compli-ntors would be out of countenance; which com-

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pliments, besides that which has been printed and published in the commendations of my writings, if it were put together, would make a greater volume than either of your libels. And truly, Sir, I had never answered your Elenchus as proceeding from Dr. Wallis, if I had not considered you also as the minister to execute the malice of that sort of people that are offended with my *Leviathan*.

As for the judgment of that public Professor that makes himself a witness of the goodness of your geometry, a man may easily see by the letter itself that he is a dunce. And for the English person of quality whom I know not, I can say no more yet than I can say of all three, that he is so ill a geometrician, as not to detect those gross paralogisms as infer that 5 to 4 and 1 to 12 are the same proportion. He came into the cry of those whom your title had deceived.

And now I shall let you see that the composition of proportion by multiplication, as it is in the fifth definition of the sixth element, is but another way of adding proportions one to another. Let the proportions be of 2 to 3, and of 4 to 5. Multiply 2 into 4 and 3 into 5, the proportion arising is of 8 to 15. Put in order these three quantities, 8, 12, 15. The proportion therefore of 8 to 15, compounded of the proportions of 8 to 12, (that is, of 2 to 3) and of 12 to 15, that is, of 4 to 5 by addition. Again, let the proportion be of 2 to 3, and of 4 to 5, multiply 2 into 5 and 3 into 4, the proportions arising is of 10 to 12. Put in order these three numbers, 10, 8, 12. The proportion 10 to 12 is compounded of the proportions of 10 to 8, that is of 5 to 4, and of 8 to 12, that is, of 2 to 3 by addition. I wonder you know not this.

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I find not any more clamour against me for saying the proportion of 1 to 2 is double to that of 1 to 4.

Your book, you speak of, concerning proportion against *Meibomius* is like to be very useful when neither of you both do understand what proportion is.

You take exceptions, as that I say, that *Euclid* has but one word for *double* and *duplicate;* which nevertheless was said very truly, and that word is sometimes διπλάσιος and sometimes διπλασίων. And you think you have come off handsomely with asking me whether διπλάσιος and διπλασίων be one word.

Nor are you now of the mind you were, that a point is not *quantity unconsidered*, but that in an infinite series it may be safely neglected. What is *neglected* but unconsidered.

Nor do you any more stand to it, that the *quotient* is the *proportion*. And yet were these the main grounds of your *Elenchus*.

But you will say, perhaps, I do answer to the defence you have now made in this your *School Discipline:* 'tis true. But 'tis not because you answer never a word to my former objections against these propositions 19, 89; but because you do so shift and wriggle, and throw out ink, that I cannot perceive which way you go, nor need I, especially in your vindication of your *Arithmetica Infinitorum*. Only I must take notice that in the end of it, you have these words, "Well, *Arithmetica Infinitorum is come off clear*." You see the contrary. For sprawling is no defence.

It is enough to me that I have clearly demonstrated both before sufficiently, and now again

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abundantly, that your book of *Arithmetica Infinitorum* is all nought from the beginning to the end, and that thereby I have effected that your authority shall never hereafter be taken for a prejudice. And, therefore, they that have a desire to know the truth in the questions between us, will henceforth, if they be wise, examine my geometry, by attentive reading me in my own writings, and then examine, whether this writing of yours confute or enervate mine.

There is in my fifth lesson a proposition, with a diagram to it, to make good, I dare say, at least against you, my twentieth chapter concerning the dimension of a circle. If that demonstration be not shown to be false, your objections to that chapter, though by me rejected, come to nothing. I wonder why you pass it over in silence. But you are not, you say, bound to answer it. True, nor yet to defend what you have written against me.

Before I give over the examination of your geometry, I must tell you that your words, (p. 101 of your *School Discipline*), against the first corollary are untrue.

Your words are these: "*you affirm that the proportion of the parabola A B I to the parabola A F K is triplicate to the proportion of the time A B to A F, as it is in the English.*" This is not so. Let the reader turn to the place and judge. And going on you say, "*or of the impetus B I to F K as it is in the Latin.*" Nay, as it is in the English, and the other in the Latin. It is but your mistake; but a mistake is not easily excused in a false accusation.

Your exception to my saying, "*that the dif-*

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ferences of two quantities is their proportion," (when they differ, as the no difference, when they be equal), might have been put in amongst other marks of your not sufficiently understanding the Latin tongue. *Differre* and *differentia* differ no more than *vivere* and *vita*, which is nothing at all, but as the other words require that go with them, which other words you do not much use to consider. But *differre* and *the quantity by which they differ*, are quite of another kind. *Differre* (τὸ διαφέρειν, τὸ ὑπερέχειν) *differing*, *exceeding*, is not quantity, but relation. But the quantity by which they differ is always a certain and determined quantity, yet the word *differentia* serves for both, and is to be understood by the coherence with that which went before. But I had said before, and expressly to prevent cavil, that relation is nothing but a comparison, and that proportion is nothing but relation of quantities, and so defined them, and therefore I did there use the word *differentia* for *differing*, and not for the quantity which was left by subtraction. For a quantity is not a *differing*. This I thought the intelligent reader would of himself understand without putting me, instead of *differentia*, to use (as some do, and I shall never do) the mongrel word τὸ *differre*. And whereas in one only place for *differre ternario* I have writ *ternarius*, if you had understood what was clearly expressed before, you might have been sure it was not my meaning, and therefore the excepting against it was either want of understanding, or want of candour, choose which you will.

You do not yet clear your doctrine of *condensation* and *rarefaction*. But I believe you will by

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degrees become satisfied that they who say the same numerical body may be sometimes greater, sometimes less, speak absurdly, and that *condensation* and *rarefaction* here, and *definitive* and *circumscriptive*, and some other of your distinctions elsewhere are but snares, such as school divines have invented

> —— ὥσπερ ἀράχνης
> Ὀυλόμενος χέζει ἀλύσεις μυίαις ἀϐαρέσσι,

to entangle shallow wits.

And that that distinction which you bring here, "*that it is of the same quantity while it is in the same place, but it may be of a different quantity when it goes out of its place*," (as if the place added to, or took any quantity from the body placed), is nothing but mere words. It is true that the body which swells changeth place, but it is not by becoming itself a greater body, but by admixtion of air or other body, as when water riseth up in boiling, it taketh in some parts of air. But seeing the first place of the body is to the body equal, and the second place equal to the same body, the places must also be equal to one another, and consequently the dimensions of the body remain equal in both places.

Sir, when I said that such doctrine was taught in the Universities, I did not speak against the Universities, but against such as you. I have done with your geometry, which is one ϛιγμὴ.

RURAL LANGUAGE.

As for your eloquence, let the reader judge whether yours or mine be the more *muddy*, though I in plain scolding should have outdone you, yet arks of Dr. Vallis's rural nguage, &c.

I have this excuse which you have not, that I did but answer your challenge at that weapon which you thought fit to choose. The catalogue of the hard language which you put in at pages 3 and 4 of your *School Discipline*, I acknowledge to be mine, and would have been content you had put in all. The titles you say I give you of *fools*, *beasts*, and *asses*, I do not give you, but drive back upon you, which is no more than not to own them; for the rest of the catalogue, I like it so well as you could not have pleased me better than by setting those passages together to make them more conspicuous; that is all the defence I will make to your accusations of that kind.

And now I would have you to consider whether you will make the like defence against the faults that I shall find in the language of your *School Discipline*.

I observe, first, the facetiousness of your title-page, "*Due correction for Mr. Hobbes, or School Discipline, for not saying his Lessons right.*" What a quibble is this upon the word lesson; besides, you know it has taken wind; for you vented it amongst your acquaintance at Oxford then when my *Lessons* were but upon the press. Do you think if you had pretermitted that piece of wit, the opinion of your judgment would have been ere the less? But you were not content with this, but must make this metaphor from the rod to take up a considerable part of your book, in which there is scarce anything that yourself can think wittily said besides it. Consider also these words of yours: "*It is to be hoped that in time you may come to learn the language, for you be come to great* A *already.*" And presently after,

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"*were I great* A, *before I would be willing to be so used, I should wish myself little* a *a hundred times*." Sir, you are a doctor of divinity and a professor of geometry, but do not deceive yourself, this does not pass for wit in these parts, no, nor generally at Oxford; I have acquaintance there that will blush at the reading it.

Again, in another place you have these words: "*Then you catechize us, 'what is your name? Are you geometricians? Who gave you that name,'*" &c. Besides in other places such abundance of the like insipid conceits, as would make men think, if they were no otherwise acquainted with the University but by reading your books, that the dearth there of salt were very great. If you have any passage more like to salt than these are (excepting *now and anon*) you may do well to show it to your acquaintance, lest they despise you; for, since the detection of your geometry, you have nothing left you else to defend you from contempt. But I pass over this kind of eloquence, and come to somewhat yet more rural.

Page 27, line 1, you say I have given Euclid his *lurry*. And again, page 129, line 11, "*and now he is left to learn his lurry*." I understand not the word *lurry*. I never read it before, nor heard it, as I remember, but once, and that was when a clown threatening another clown said he would give him *such a lurry come poop*, &c. Such words as these do not become a learned mouth, much less are fit to be registered in the public writings of a doctor of divinity. In another place you have these words, "*just the same to a cow's thumb*," a pretty adage.

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Page 2, "*But prithee tell me.*" And again, page 95, "*prithee tell me, why dost thou ask me such a question,*" and the like in many other places.

You cannot but know how easy it is and was for me to have spoken to you in the same language. Why did I not? Because I thought that amongst men that were civilly bred it would have redounded to my shame, as you have cause to fear that this will redound to yours. But what moved you to speak in that manner? Were you angry? If I thought that the cause, I could pardon it the sooner, but it must be very great anger that can put a man, that professeth to teach good manners, so much out of his wits as to fall into such a language as this of yours. It was perhaps an imagination that you were talking to your inferior, which I will not grant you, nor will the heralds, I believe, trouble themselves to decide the question. But, howsoever, I do not find that civil men use to speak so to their inferiors. If you grant my learning but to be equal to yours, (which you may certainly do without very much disparaging of yourself abroad in the world), you may think it less insolence in me to speak so to you in respect of my age, than for you to speak so to me in respect of your young doctorship. You will find that for all your doctorship, your elders, if otherwise of as good repute as you, will be respected before you. But I am not sure that this language of yours proceeded from that cause; I am rather inclined to think you have not been enough in good company, and that there is still somewhat left in your manners for which the honest youths of Hedington and Hincsey may compare with you for good language, as great a doctor as you are.

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For my verses of the Peak, though they be as ill in my opinion as I believe they are in yours, and made long since, yet they are not so obscene as that they ought to be blamed by Dr. Wallis. I pray you, sir, whereas you have these words in your *School Discipline*, page 96, "*unless you will say that one and the same motion may be now and anon too.*" What was the reason you put these words, *now and anon too*, in a different character, that makes them to be more taken notice of? Do you think that the story of the minister that uttered his affection (if it be not a slander) not unlawfully but unseasonably, is not known to others as well as to you? What needed you then, when there was nothing that I had said could give the occasion, to use those words; there is nothing in my verses that do *olere hircum* so much as this of yours. I know what good you can receive by ruminating on such ideas, or cherishing of such thoughts. But I go on to other words of mine by you reproached, "*you may as well seek the focus of the parabola of Dives and Lazarus*," which you say is mocking the Scripture; to which I answer only, that I intended not to mock the Scripture, but you, and that which was not meant for mocking was none. And thus you have a second ϛιγμὴ.

GRAMMAR AND CRITIQUES.

I come now to the comparison of our Grammar and Critiques. You object first against the signification I give of ϛιγμὴ, and say thus: "*What should come into your cap* (that, if you mark it, in a man that wears a square cap to one that wears a hat, is very witty) *to make you think that* ϛιγμὴ *signi-*

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fies a mark or brand with a hot iron? I perceive where the business lies, it was στίγμα *run in your mind when you talked of* στιγμή*; and because the words are somewhat alike you jumble them both together.*" Sir, I told you once before, you presume too much upon your first cogitations. Aristophanes, in *Ranis*, Act. v. Scen. 5,

> Κἂν μὴ ταχέως ἥκωσι
> Νὴ τεν 'Απόλλο στίξας ἀυτοὺς.

The old commentator upon the word στίξας saith thus, στίξας ἀντὶ τοῦ στιγματίσας, ἦν γάρ ξένος. That is, στίξας for στιγματίσας, for he (Adimantus) was not a citizen. I hope the commentator does not here mock Aristophanes for jumbling στίξας and στιγματίσας together, for want of understanding Greek. No, στίξας and στιγματίσας signify the same, save that for branding I seldom read στιγματίσας but στίξας. For στίγμα does no more signify a brand with a hot iron, than στιγμή a point made also with a hot iron. They have both one common theme στίζω, which does not signify *pungo*, nor *interpungo*, nor *inuro*, for all your Lexicon, but *notam imprimere*, or *pungendo notare*, without any restriction to burning or punching. It is therefore no less proper to say that στιγμή is a mark with a hot iron, than to say the same of στίγμα. The difference is only this, that when they marked a slave, or a rascal, as you are not ignorant is usually done here at the assizes in the hand or shoulder with a hot iron, they called that στίγμα, not for the burning, but for the mark. And as it would have been called στίγμα that was imprinted on a slave, though made by staining or incision, so it is στιγμή, though done with a hot iron. And therefore there was no jumbling of those two words together, as for want of reading Greek authors, and

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by trusting too much to your dictionaries, which you say are proofs good enough for such a business, you were made to imagine. The use I have made thereof was to show that a point, both by the word Σημε͂ον in Euclid, and by the word ϛιγμὴ in some others, was not *nothing*, but a *visible* mark, the ignorance whereof hath thrown you into so many paralogisms in geometry.

But do you think you can defend your *Adducis Malleum* as well as I have now defended my ϛιγμὴ? You have brought, I confess, above a hundred places of authors, where there is the word *duco*, or some of its compounds, but none of them will justify *Adducis Malleum*, and, excepting two of those places, you yourself seem to condemn them all, comparing yours with none of the rest but with these two only, both out of Plautus, by you not well understood. The first is in *Casina*, Act. v. Scen. 2, "*Ubi intro hanc novam nuptam deduxi, via recta, clavem abduxi*;" which you, presently presuming of your first thoughts, a peculiar fault to men of your principles, assure yourself is right. But if you look on the place as Scaliger reads it, cited by the commentator, you will find it should be *obduxi*, and that *clavis* is there used for the bolt of the lock. Besides, he bolted it within. Whither then could he carry away the key? The place is to be rendered thus, *when I had brought in this new bride I presently locked the door*, and is this *as bad every whit* as *Adducis Malleum?* The second place is in *Amphytruo*, Act. I. Scen. 1, "*Eam* (cirneam), *ut a matre fuerat natum, plenam vini eduxi meri*," which you interpret *I brought out a flagon of wine*, unlearnedly. They are

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the words of Mercury transformed into Sosia. And to try whether Mercury were Sosia or not, Sosia asked him where he was and what he did during the battle; to which Mercury answered, who knew where Sosia then was and what he did, *I was in the cellar, where I filled a cirnea, and brought it up full of wine, pure as it came from its mother*. By the mother of the wine meaning the vine, and alluding to the education of children, for *ebibi* said *eduxi*, and with an *emphasis* in *meri*, because *cirnea* (from Κιρνάω, *misceo*) was a vessel wherein they put water to temper to their wine. Intimating that though the vessel was *cirnea*, yet the wine was *merum*. This is the true sense of the place; but you will have *eduxi* to be, *I brought out*, though he came not out himself. You see, sir, that neither this is so bad as *Adducis Malleum*.

But suppose out of some one place in some one blind author you had paralleled your *Adducis Malleum*, do you think it must therefore presently be held for good Latin? Why more than *learn his lurry* must be therefore thought good English a thousand years hence, because it will be read in Dr. Wallis's long-lived works. But how do you construe this passage (1 Tim. ii. 15) of the Greek Testament: Σωθήσεται δὲ διὰ τῆς τεκνογονίας, ἐὰν μείνωσιν ἐν πίϛει? You construe it thus: *she shall be saved notwithstanding child-bearing, if (the woman) remain in the faith*. Is child-bearing any obstacle to the salvation of women? You might as well have translated the first verse of the fifth of Romans in this manner, *Being then justified by faith, we have peace with God notwithstanding our Lord Jesus Christ*. I let pass your not finding in τεκνογονίας, as good a

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grammarian as you are, a nominative case to *μείνωσιν.* If you had remembered the place, 1 Pet. iii. 20, εσώθησαν δὶ ὕδατος, that is, *they were saved in the waters*, you would have thought your construction justified then very well; but you had been deceived, for διὰ does not there signify *causam, ablationem impedimenti*, but *transitum*; not *cause or removing an impediment*, but *passage*. Being come thus far, I found a friend that hath eased me of this dispute; for he showed me a letter written to himself from a learned man, that hath out of very good authors collected enough to decide all the grammatical questions between you and me, both Greek and Latin. He would not let me know his name, nor anything of him but only this, that he had better ornaments than to be willing to go clad abroad in the habit of a grammarian. But he gave me leave to make use of so much of the letter as I thought fit in this dispute, which I have done, and have added it to the end of this writing. But before I come to that, you must not take it ill, though I have done with your *School Discipline*, if I examine a little some other of your printed writings as you have examined mine; for neither you in geometry, nor such as you in church politics, cannot expect to publish any unwholesome doctrine without some antidotes from me, as long as I can hold a pen. But why did you answer nothing to my sixth *Lesson?* Because, you say, it concerned your colleague only. No, sir, it concerned you also, and chiefly, for I have not heard that your colleague holdeth those dangerous principles which I take notice of in you, in my sixth *Lesson*, page 350, upon the occasion of these

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words, not his but yours: "*Perhaps you take the whole history of the fall of Adam for a fable, which is no wonder, seeing you say the rules of honouring and worshipping of God are to be taken from the laws.*" In answer to which I said thus: "*You that take so heinously, that I would have the rule of God's worship in a Christian commonwealth to be taken from the laws, tell me from whom you would have them taken? From yourself? Why so, more than from me? From the bishops? Right, if the supreme power of the commonwealth will have it so; if not, why from them rather than from me? From a consistory of presbyters themselves, or joined with lay elders, whom they may sway as they please? Good, if the supreme governor of the commonwealth will have it so. If not, why from them rather than from me, or from any man else? They are wiser and learneder than I; it may be so, but it has not yet appeared. Howsoever, let that be granted. Is there any man so very a fool as to subject himself to the rules of other men in those things which do so nearly concern himself, for the title they assume of being wise and learned, unless they also have the sword which must protect them? But it seems you understand the sword as comprehended. If so, do not you then receive the rules of God's worship from the civil power? Yes, doubtless; and you would expect, if your consistory had that sword, that no man should dare to exercise or teach any rules concerning God's worship which were not by you allowed.*"

This will be thought strong arguing, if you do not answer it. But the truth is, you could say nothing against it without too plainly discovering your disaffection to the government. And yet you have discovered it pretty well in your second *Thesis*, maintained in the Act at Oxford, 1654, and since by yourself published. This *Thesis* I shall speak briefly to.

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SCOTCH CHURCH POLITICS.

You define ministers of the Gospel to be those *to whom the preaching of the Gospel by their office is enjoined by Christ*. Pray you, first, what do you mean by saying preaching *ex officio is enjoined by Christ?* Are they preachers *ex officio*, and afterwards enjoined to preach? *Ex officio* adds nothing to the definition; but a man may easily see your purpose to disjoin yourself from the state by inserting it.

Secondly, I desire to know in what manner you will be able out of this definition to prove yourself a minister? Did Christ himself immediately enjoin you to preach, or give you orders? No. Who then, some bishop, or minister, or ministers? Yes; by what authority? Are you sure they had authority immediately from Christ? No. How then are you sure but that they might have none? At least, some of them through whom your authority is derived might have none. And therefore if you run back for your authority towards the Apostles' times but a matter of sixscore years, you will find your authority derived from the Pope, which words have a sound very unlike to the voice of the laws of England. And yet the Pope will not own you. There is no man doubts but that

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you hold that your office comes to you by successive imposition of hands from the time of the Apostles; which opinion in those gentle terms passeth well enough; but to say you derive your authority from thence, not through the authority of the sovereign power civil, is too rude to be endured in a state that would live in peace. In a word, you can never prove you are a minister, but by the supreme authority of the commonwealth. Why then do you not put some such clause into your definition? As thus, *ministers of the Gospel are those to whom the preaching of the Gospel is enjoined by the sovereign power in the name of Christ.* What harm is there in this definition, saving only it crosses the ambition of many men that hold your principles? Then you define the power of a minister thus: "*The power of a minister is that which belongeth to a minister of the Gospel in virtue of the office he holds, inasmuch as he holds a public station, and is distinguished from private Christians. Such as is the power of preaching the Gospel, administering the sacrament, the use of ecclesiastical censures, and ordaining of ministers,*" *&c.*

Again, how will you prove out of this definition that you, or any man else, hath the power of a minister, if it be not given him by him that is the sovereign of the commonwealth? For seeing, as I have now proved, it is from him that you must derive your ministry, you can have no other power than that which is limited in your orders, nor that neither longer than he thinks fit. For if he give it you for the instruction of his subjects in their duty, he may take it from you again whensoever he shall see you instruct them with undutiful

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and seditious principles. And if the sovereign power give me command, though without the ceremony of imposition of hands, to teach the doctrine of my *Leviathan* in the pulpit, why am not I, if my doctrine and life be as good as yours, a minister as well as you, and as public a person as you are? For *public person*, primarily, is none but the civil sovereign, and so secondarily, all that are employed in the execution of any part of the public charge. For all are his ministers, and therefore also Christ's ministers because he is so; and other ministers are but his vicars, and ought not to do or say anything to his people contrary to the intention of the sovereign in giving them their commission.

Again, if you have in your commission a power to excommunicate, how can you think that your sovereign who gave you that commission, intended it for a commission to excommunicate himself? that is, as long as he stand excommunicate, to deprive him of his kingdom. If all subjects were of your mind, as I hope they will never be, they will have a very unquiet life. And yet this has, as I have often heard, been practised in Scotland, when ministers holding your principles had power enough, though no right, to do it.

And for administration of the sacraments, if by the supreme power of the commonwealth it were committed to such of the laity as know how it ought to be done as well as you, they would *ipso facto* be ministers as good as you. Likewise the right of ordination of ministers depends not now on the imposition of hands of a minister or presbytery, but on the authority of the Christian sovereign, Christ's immediate vicar and supreme

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governor of all persons and judge of all causes, both spiritual and temporal, in his own dominions, which I believe you will not deny.

This being evident, what acts are those of yours which you call *authoritative*, and receive not from the authority of the civil power? A constable does the acts of a constable *authoritatively* in that sense. Therefore you can no otherwise claim your power than a constable claimeth his, who does not exercise his office in the constabulary of another. But you forget that the Scribes and the Pharisees sit no more in Moses' chair.

You would have every minister to be a minister of the universal Church, and that it be lawful for you to preach your doctrine at Rome; if you would be pleased to try, you would find the contrary. You bring no argument for it that looks like reason. Examples prove nothing, where persons, times, and other circumstances differ; as they differ very much now when kings are Christians, from what they were then when kings persecuted Christians. It is easy to perceive what you aim at.

You would fain have market-day lectures set up by authority, (not by the authority of the civil power, but by the authority of example of the Apostles in the emission of preachers to the infidels), not knowing that any Christian may lawfully preach to the infidels; that is to say, proclaim unto them that Jesus is the Messiah, without need of being otherways made a minister, as the deacons did in the Apostles' time; nor that many teachers, unless they can agree better, do anything else but prepare men for faction, nay, rather you know it well enough, but it conduces to your end

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upon the market-days to dispose at once both town and country, under a false pretence of obedience to God, to a neglecting of the commandments of the civil sovereign, and make the subject to be wholly ruled by yourselves, wherein you have already found yourselves deceived. You know how to trouble and sometimes undo a slack government, and had need to be warily looked to, but are not fit to hold the reins. And how should you, being men of so little judgment as not to see the necessity of unity in the governor, and of absolute obedience in the governed, as is manifest out of the place of your *Elenchus* above recited. The doctrine of the duty of private men in a commonwealth is much more difficult, not only than the knowledge of your symbols, but also than the knowledge of geometry itself. How then do you think, when you err so grossly in a few equations, and in the use of most common words, you should be fit to govern so great nations as England, Ireland, and Scotland, or so much as to teach them? For it is not reading but judgment that enables one man to teach another.

I have one thing more to add, and that is the disaffection I am charged withal to the universities. Concerning the Universities of Oxford and Cambridge, I ever held them for the greatest and noblest means of advancing learning of all kinds, where they should be therein employed, as being furnished with large endowments and other helps of study, and frequented with abundance of young gentlemen of good families and good breeding from their childhood. On the other side, in case the same means and the same wits should be employed in the advancing of the doctrines that tend

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to the weakening of the public, and strengthening of the power of any private ambitious party, they would also be very effectual for that; and consequently that if any doctrine tending to the diminishing of the civil power were taught there, not that the Universities were to blame, but only those men that in the universities, either in lectures, sermons, printed books, or theses, did teach such doctrine to their hearers or readers. Now you know very well that in the time of the Roman religion, the power of the Pope in England was upheld principally by such teachers in the universities. You know also how much the divines that held the same principles in Church government with you, have contributed to our late troubles. Can I therefore be justly taxed with disaffection to the universities for wishing this to be reformed? And it hath pleased God of late to reform it in a great measure, and indeed as I thought totally, when out comes this your *Thesis* boldly maintained to show the contrary. Nor can I yet call this your doctrine the doctrine of the university; but surely it will not be unreasonable to think so, if by public act of the university it be not disavowed, which done, and that as often as there shall be need, there can be no longer doubt but that the universities of England are not only the noblest of all Christian universities, but also absolutely, and of the greatest benefit to this commonwealth that can be imagined, except that benefit of the head itself that uniteth and ruleth all. I have not here particularized at length all the ill consequences that may be [illegible] from this *Thesis* of yours, because [illegible] further provoked, have somewhat to [illegible]. So much for the third στιγμὴ.

AN EXTRACT OF A LETTER CONCERNING THE GRAMMATICAL PART OF THE CONTROVERSY BETWEEN MR. HOBBES AND DR. WALLIS.

An extract of a letter concerning the grammatical part of the controversy, &c.

Mr. Hobbes hath these words: "*Longitudinem percursam motu uniformi, cum impetu ubique ipsi B D æquali.*" Dr. Wallis saith *cum* were better out, unless you would have *impetus* to be only a companion, not a *cause*. Mr. Hobbes answered it was the *ablative case of the manner*. The truth is the ablative case of the *manner* and *cause* both, may be used with the conjunction *cum*, as may be justified. Cicero in Lib. II. *De Nat. Deorum:* "*Moliri aliquid cum labore operoso ac molesto;*" and in his oration for Cæcina: "*De se autem hoc prædicat, Antiocho Ebulii servo imperasse ut in Cæcinam advenientem cum ferro invaderet.*" Let us see then what Dr. Wallis objects against Tully, where a casualty is imported, though we may use *with* in English, yet not *cum* in Latin; to kill with a sword, importing this to have an instrumental or causal influence, and not only that it hangs by the man's side whilst some other weapon is made use of, is not in Latin *occidere cum gladio*, but *gladio occidere*. This shows that the Doctor hath not forgot his grammar, for the subsequent examples as well as this rule are borrowed thence. But yet he might have known that great personages have never confined themselves to this pedantry, but have chosen to walk in a greater latitude. Most of the elegancies and idioms of every language are exceptions to his grammar. But since Mr. Hobbes saith it is the ablative case of

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the manner, there is no doubt it may be expressed with *cum*. The Doctor in the meantime knew no more than what Lilly had taught him; Alvarez would have taught him more; and Vossius in his book, *De Constructione, cap.* XLVII. expressly teacheth, "*Ablativos causæ, instrumenti, vel modi, non a verbo regi sed a præpositione omissa, a vel ab, de, e vel ex, præ, aut cum, ac præpositiones eas quandoque exprimi nisi quod cum ablativis instrumenti haud temere invenias;*" and afterwards he saith, "*non timere imitandum.*" If this be so, then did Mr. Hobbes speak grammatically, and with Tully, but not *usually*. And might not one retort upon the Doctor, that Vossius is as great a critic as he?

His next reflection is upon *prætendit scire*, this he saith is an Anglicism. If this be all his accusation, upon this score we shall lose many expressions that are used by the best authors, which I take to be good Latinisms, though they be also Anglicisms, the latter being but an imitation of the former. The Doctor therefore was too fierce to condemn upon so general an account, that which was not to have been censured for being an Anglicism, unless also it had been no Latinism. Mr. Hobbes replies, that the printer had omitted *se*. He saith, this mends the matter a little. It is very likely, for then it is just such another Anglicism as that of Quintilian: "*Cum loricatus in ro ambularet, prætendebat se id metu facere.*" e Doctor certainly was very negligent, or else could not have missed this in Robert Stephen. haply he was resolved to condemn Quintilian this and that other Anglicism, "*Ignorantia tendi non potest,*" as all those that have used *prætendo*, which are many and as good authors as Dr. Wallis, that makes his own encomiasts (not an Englishman amongst them) to write Anglicisms.

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Then he blames "*Tractatus hujus partis tertiæ, in qua motus et magnitudo per se et abstracte consideravimus, terminum hic statuo.*" Here I must confess the exception is colourable, yet I can parallel it with the like objection made by Erasmus against Tully, out of whom Erasmus quotes this passage: "*Diutius commorans Athenis, quoniam venti negabant solvendi facultatem, erat animus ad te scribere;*" and excuses it thus, that Tully might have had at first in his thoughts *volebam* or *statuebam*, which he afterwards relinquished for *erat animus*, and did not remember what he had antecedently written, which did not vary from his succeeding thoughts, but words. And this excuse may pass with any who know that Mr. Hobbes values not the study of words, but as it serves to express his thoughts, which were the same whether he wrote *in qua motus et magnitudo per se at abstracte considerati sunt* or *consideravimus*. And if the Doctor will make this so capital, he must prove it *voluntary*, and show that it is greater than what is legible in the puny letter of his encomiast, whom he would have to be beyond exception.

Now follows his ridiculous apology for *adducis malleum, ut occidas muscam.* The cause why he did use that proverb, of his own phrasing, was this. Mr. Hobbes had taken a great deal of pains to demonstrate what Dr. Wallis thought he could have proved in short; upon this occasion he objects, *adducis malleum ut occidas muscam*, which I shall suppose he intended to English thus, *you bring*

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a beetle to kill a fly. Mr. Hobbes retorted, that *adduco* was not used in that sense. The Doctor vindicates himself thus: *duco, deduco, reduco, perduco, produco,* &c. signify the same thing, ergo, *adduco* may be used in that sense; which is a most ridiculous kind of arguing, where we are but to take up our language from others, and not to coin new phrases. It is not the grammar that shall secure the Doctor, nor weak analogies, where elegance comes in contest. To justify his expression he must have showed it *usu tritum,* or alleged the authority of some author of great note for it. I have not the leisure to examine his impertinent citations about those other compounds, nor yet of that simple verb *duco;* nay, to justify his saying he hath not brought one parallel example. He talks indeed very high, that *duco,* with its compounds, is a word of a large signification, and amongst the rest *to bring, fetch, carry,* &c. is so exceeding frequent in all authors, Plautus, Terence, Tully, Cæsar, Tacitus, Pliny, Seneca, Virgil, Horace, Ovid, Claudian, &c. that he must needs be either maliciously blind, or a very stranger to the Latin tongue, that doth not know it, or can have the face to deny it. I read, what will be my doom for not allowing his Latin; yet I must profess I dare secure the Doctor for having read all authors, notwithstanding his assertion, and I hope
vill do the like for me. And for those which
ith read, had he brought no better proofs than
he had, I am sure, been whipped soundly in
ninster School, for his impudence as well as
nce, by the learned master thereof at pre-
But I dare further affirm, the Doctor hath not

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read in this point any, but only consulted with Robert Stephen's *Thesaurus Linguæ Latinæ*, whence he hath borrowed his allegations in *adduco;* and for the other, I had not so much idle time as to compare them. And, lest the fact might be discovered, he hath sophisticated those authors whence Stephen cites the expressions, and imposed upon them others. If it be not so, or that the Doctor could not write it right when the copy was right before him, let him tell me where he did ever read in Plautus, *adducta res in fastidium.* I find the whole sentence in Pliny's preface to Vespasian (out of whom in the precedent paragraph he cites it) about the middle: *alia vero ita multis prodita, ut in factidium sint adducta*, which is the very example Stephanus useth, although he doth premise his *adducta res in fastidium.* Let the Doctor tell where he ever did read in Horace, *Ova noctuæ*, &c. *tædium vini adducunt.* Did he, or any else, with the interposition of an &c. make Trochaics? I say, and Stephanus says so, too, that it is in Pliny, lib. xiii. cap. 15, near the end; the whole sentence runs thus: *Ebriosis Ova noctuæ per triduum data in vino, tædium ejus adducunt.* I doubt not but these are the places he aimed at, although he disguised and minced the quotations; if they be not, I should be glad to augment my Stephanus with his additions.

These things premised, I come to consider the Doctor's proofs: *Res eo adducta est: adducta vita in extremum: adducta res in fastidium: rem ad mucrones et manus adducere: contractares et adducta in augustum: res ad concordiam adduci potest: in ordinem adducerem: adducere febres,*

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sitim, tedium vini (all in Robert Stephen) betwixt which and *adducere malleum*, what a vast difference there is, I leave them to umpire *qui terretes et religiosas nacti sunt aures*, who are the competent judges of elegancy, and only cast in the verdict of one or two, who are in any place (where the purity of the Latin tongue flourisheth) of great esteem. Losæus, in his *Scopæ Linguæ Latinæ, ad purgandam Linguam a barbarie*, &c. (would any think that the Doctor's elegant expression, frequent in all authors, which none but the malicious or ignorant can deny, should suffer so contumelious an expurgation?) Losæus, I say, hath these words: *Adferre plerique minus attenti utuntur pro adducere. Quod Plautus, in Pseudolo, insigni exemplo notat.*

> *CA.*—Attuli hunc.
> *PS.*—Quid attulisti?
> *CA.*—Adduxi volui dicere.
> *PS.*—Quis istic est?
> *CA.*—Charinus.

Satis igitur admonet discriminis inter ducere, reducere, adducere, et abducere, quæ de persona; et ferre, adferre, &c. *quæ de re dicuntur.* Idem, *Demetrium, quem ego novi, adduce: argentum non moror quin feras. Cavendum igitur est ne vulgi more,* (let the Doctor mark this, and know that *this author is authentic amongst the Ciceronians*), *adferre de persona, dicamus, sed adducere; licet et hoc de certis quibusdam rebus non inepte dicatur.* In this last clause he saith as much as Mr. Hobbes saith, and what the Doctor proves; but, that ever the Doctor brought an example which might resemble *adducis malleum*, is denied; for I have mentioned already his allega-

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tions, every one, of *adduco*. Another author, (a fit antagonist for the elegant Doctor), is the *Farrago sordidorum Verborum*, joined with the Epitome of L. Valla's *Elegancies*. He saith: *Accerse, adhuc Petrum, Latine dicitur, pro eo quod pueri dicunt, adfer Petrum.* And this may suffice to justify Mr. Hobbes's exception who proceeded no further than this author to tell the Doctor that *adduco* was used of animals. But the Doctor replies, *this signification is true, but so may the other be also.* I say if it never have been used so, it cannot be so, for we cannot coin new Latin words, no more than French or Spanish who are foreigners. Mr. Hobbes was upon the negative, and not to disprove the contrary opinion. If the Doctor would be believed, he must prove it by some example, (which is all the proof of elegancy), and till he do so, not to believe him, it is sufficient not to have cause. But, Doctor Wallis, *why not adduco for a hammer as well as a tree?* I answer yes, equally for either, and yet for neither. Did ever anybody go about to mock his readers thus solemnly? I do not find, to my best remembrance, any example of it in Stephen, and the Doctor is not wiser than his book; if there be, it is strange the Doctor should omit the only pertinent example, and trouble us with such impertinences for three or four pages. In Stephen there are *adducere habenas* and *adducere lorum*, but in a different sense. It is not impossible I may guess at the Doctor's aim. In Tully *de Nat. Deor.* as I remember, there is this passage: *Quum autem ille respondisset, in agro ambulanti ramulum adductum ut remissus esset, in oculum suum recidisse*, where it signifies nothing

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else but to be *bent*, *bowed*, *pulled back*, and in that sense, *the hammer of a clock*, or that of a *smith*, *when he fetcheth his stroke*, may be said *adduci*. And this, I conceive, the Doctor would have us in the close think to have been his meaning; else, what doth he drive at in these words? "When you have done the best you can, you will not be able to find better words than *adducere malleum* and *reducere*, to signify the two contrary motions of the *hammer*, the one when you strike with it (*excellently trivial!*) the other when you take it back (*better and better*), *What to do?* to fetch another stroke. If any can believe that this was his meaning, I shall justify his Latin, but must leave it to him to prove its sense. If he intended no more, why did he go about to defend the other meaning, and never meddle with this? Which yet might have been proved by this one example of mine? May not, therefore, his own saying be justly retorted upon him in this case, *Adducis malleum, ut occidas muscam*?

Another exception is, *Falsæ sunt, et multa istiusmodi* (*propositiones*). I wish the Doctor could bring so good parallels, and so many, out of any author, for his *Adducis malleum*, as Tully affords in this case. Take one for all, out of the beginning of his *Paradoxes: Animadverti sæpe Catonem, cum in senatu sententiam diceret, Locos graves ex Philosophia tractare, abhorrentes ab hoc usu forensi, et publico, sed dicendo consequi tamen, ut illa etiam populo probabilia viderentur.* This is but a *Solæcophanes*, and hath many precedents more, as in the second book of his *Academical Questions*, &c.

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I cannot now stay upon each particular passage; I do not see any necessity of tracing the Doctor in all his vagaries. Now, he disallows *tanquam diceremus, as if we should say*. But why is that less tolerable than *tanquam feceris, as if you had done?* "It should be *quasi*, (forsooth!) or *ac si*, or *tanquam si*, which is Tully's own word." What is *tanquam si* become but one word? *Tanquam si tua res agatur*, &c. Good Doctor, leave out Tully and all *Ciceronians*, or you will for ever suffer for this, and your *Adducis malleum*. Is not this to put yourself on their verdict when you oppose Mr. Hobbes with Tully? But the Doctor gives his reason. And though he hath had the luck in his *Adducis malleum*, to follow the first part of that saying, *Loquendum cum vulgo*, yet now it is, *sentiendum cum sapientibus*. For *tanquam* without *si* signifies but *as*, not *as if*. It is pity the Doctor could not argue in symbols too, that so we might not understand him; but suppose all his papers to carry evidence with them, because they are *mathematically* scratched. How does he construe this:—

> "Plance tumes alto Drusorum sanguine, tanquam
> Feceris ipse aliquid, propter quod nobilis esses."

So Cœlius, one much esteemed by Cicero, who hath inserted his Epistles into his works, saith, in his fifth Epistle (Tul. Epist. Fam. lib. viii. ep. 5), *Omnia desiderantur ab eo tanquam nihil denegatum sit ei quo minus paratissimus esset qui publico negotio præpositus est*. But it was not possible the Doctor should know this, it not being in Stephen, where his examples for *tanquam si* are.

But, the Doctor having pitched upon this criticism, and penned it, somebody, I believe, put him

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in mind of the absurdity thereof; and yet the generous *Professor*, (who writes running hand and never transcribed his papers, if I am not misinformed), presumed nobody else could be more intelligent than he, who had perused Stephen. He would not retract anything, but subjoins, "That he will allow it as passable, because other modern writers, and some of the ancients, have so used it, as Mr. Hobbes hath done." I know not what authors the Doctor meant, for, if I am not much mistaken, I do not find any in Stephen. His citation of Columella is not right, (lib. v. cap. 5), nor can I deduce anything thence till I have read the passage, but, if he take Juvenal and Cœlius for modern authors, I hope he will admit of Accius, Nævius, and Carmenta, for the only ancients. Let him think upon this criticism, and never hope pardon for his *Adducis malleum*, which is not half so well justified, and yet none but *madmen* or *fools* reject it.

But certainly the Doctor should not have made it his business to object *Anglicisms*, in whose Elenchus I doubt not but there may be found such phrases as may serve to convince him that he is an Englishman, however Scottified in his principles. If the Doctor doubt of it, or but desire a catalogue, let him but signify his mind, and he shall be furnished with a *Florilegium*. But I am now come to the main controversy about Empusa. The Doctor saith nothing in defence of his *quibble*, nor gives any reason why he jumbled languages to make a silly clinch, which will not pass for wit either at Oxford or at Cambridge; no, nor at Westminster.

It seems he had derived *Empusa* from ἓν and ποῦς,

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and said it was a kind of *Hobgoblin* that hopped upon one leg: and hence it was that the boys' play (*Fox come out of thy hole*) came to be called *Empusa*. I suppose he means *Ludus Empusæ*. This derivation he would have to be good, and that we may know his reading, (though he hath scarce consulted any of the authors), he saith Mr. Hobbes did laugh at it, until somebody told him that it was in the Scholiast of Aristophanes (as good a critic as Mr. Hobbes), Eustathius, Erasmus, Cœlius Rhodiginus, Stephanus, Scapula, and Calepine. But sure he doth not think to scape so. To begin with the last; Calepine doth indeed say, *uno incedit pede, unde et nomen*. But he is a *Modern*, and I do not see why his authority should outweigh mine if his author's reasons do not. He refers to Erasmus and Rhodiginus. Erasmus in the adage, *Proteo mutabilior* hath these words of Empusa: *Narrant autem uno videri pedi*—this is not to hop—*unde et nomen inditum putant*, Ἔμπουσαν οἱονεὶ ἐνίποδα. He doth not testify his approbation of the derivation at all, only lets you know what etymologies some have given before him. And doth anybody think that Dr. Harmar was the first which began to show his wit, (or folly), in etymologizing words? Cœlius Rhodiginus doth not own the derivation, only saith, *Nominis ratio est, ut placet Eustathio, quia uno incedit pede* ;—is this to hop?—*sed nec desunt qui alterum interpretentur habere æneum pedem, et inde appellatam Empusam ; quod in Batrachis Aristophanes expressit*. And then he recites the interpretation that Aristophanes's Scholiast doth give upon the text, of which by and by. If any credit be to be attributed to this allegation, his last

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thoughts are opposite to Dr. Wallis; and *Empusa* must be so called, not because she hopped upon one leg, but because she had but one, the other being brass. But for the former derivation he refers to Eustathius.

As to Eustathius, I do easily conjecture that the reader doth believe that Rhodiginus doth mean Eustathius upon Homer, for that is the book of most repute and fame, his other piece being no way considerable for bulk or repute. But it is not that book, nor yet his History of Ismenias, but his notes upon the 725th verse of Dionysius Περιηγητής. The poet had said of the stone *Jaspis*, that it was

Ἐχθρὴν Ἐμπούσησι καὶ ἄλλοις ἐιδώλοισιν,

Upon which Eustathius thus remarks: Δοκεῖ γαρ ἀλεξίκακος εἶναι ἡ λίθος ἄυτη, καὶ ἀποτροπιαϛικὴ φασμάτων, ὧν ἕν ἐϛι και ἡ Ἔμπȣσα, δαιμονιόν τι τερὶ τὶω Ἑκάτην, ἑνὶ ποδὶ ζοκοῦν διήκεσθαι· (*fortè* διερείδεσθαι *Steph.*) ὅθεν καὶ παρονομάζεται, ὡς ἔι τις ἔιπη μονόπȣς ποδι ζωοῦ· ὡς τοῦ ἑτέρȣ πȣδος χαλκοῦ ὄντος, κατὰ τὸν μῦθον. This testimony doth not prove anything of *hopping*, and, as to the derivation, I cannot but say that Eustathius had too much of the grammarian in him, and this is not the first time, neither in this book, nor elsewhere, wherein he hath trifled. It is observable out of the place, that there were more *Empusas* than one, as, indeed, the name is applied by several men to any kind of frightful phantasm. And so it is used by several authors, and for as much as phantasms are various, according as the persons affrighted have been severally educated, &c. every man did impose this name upon his own apprehensions. This gave men occasion to fain *Empusa* as such—for who will believe that she was not apprehended as having four legs, when she

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appeared in the form of a cow, dog, &c.—but, as apprehended by *Bacchus* and his man at that time. I do not find that she appeared in any shape but such as made use of legs in going, whence I imagine that *Empusæ* might be opposite to the ϑεοὶ νεπόδες, which appellation was anciently fixed upon the gods, (*propitious*) upon a two-fold account; first, for that they were usually effigiated as having no feet, which is evident from ancient sculpture, and secondly, for that they are all said not to walk, but rather swim, if I may so express that *non gradiuntur, sed fluunt*, which is the assertion of all the commentators I have ever seen upon that verse of Virgil:—

> "Et vera incessu patuit dea"——

This whole discourse may be much illustrated from a passage in Heliodorus, Æthiop. lib. iii. sec. 12, 13. Calasiris told Cnemon that the Gods Apollo and Diana did appear unto him; Cnemon replied, Ἀλλὰ τίνα δὴ τρόπον ἔφασκες ἐνδεδεῖχθαί σοι τοῦς θεοῦς ὅτι μὴ ἐνύπνιον ἦλϑον, ἀλλ' ἐναργῶς ἐφᾶνησαν; upon this the old priest answered, that both gods and demons, when they appear to men, may be discovered by the curious observer, both in that they never shut their eyes, καὶ τῳ βαδίσματι πλέον, οὐ κατὰ διάϛησιν τῶν ποδῶν οὐδέ μετάϑεσιν ἀνυομένῳ, ἀλλὰ κατὰ τινα ῥῦμην ἀέριον, καὶ ὁρμὴν ἀπαραπόδιϛον, τεμνόντων μᾶλλον τὸ περιεχον ἢ διαπορευομένων. Δὶο δὴ καὶ τὰ ἀγάλματα τῶν θεῶν Αἰγύπτιοι τὼ πὸδέ ζευγνύντες καὶ ὥσπερ ἑνοῦντες ἵϛᾶσιν. ἅ δὴ καὶ Ὅμηρος ἐιδῶς, ἅτε Ἀιγύπτιος, καί τὴν ἱερὰν πάιδευσιν ἐκδιδαχθείς, συμβολικῶς τοῖς ἔπεσιν ἐναπεϑετο, τοῖς δυναμένοις συνιέναι γνωριζειν καταλιπών, ἐπι τοῦ ποσειδῶνος, το

> Ἴχνια γὰρ μετόπισϑε, ποδῶν ἠδὲ κνημάων
> Ῥεῖ ἔγνων ἀπιὸντος.

οἷον ῥέοντυς ἐν τῆ πορεία, τοῦτο γάρ εϛι τό ῥεῖ ἀπιόντος, καὶ οῦχ ὥς

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τινες ἠπάτηνται, ῥᾳδίωςἔγνων ὑπολαμβάνοντες. Farnaby, upon the place in Virgil, observes, that *Deorum incessus est continuus et æqualis, non dimotis pedibus, neque transpositis,* ἀλλὰ κατὰ ῥύμην ἀέριον. Cornelius Schrevelius in the new Leyden notes saith, *Antiquissima quæque Deorum simulachra, quod observarunt viri magni, erant* τοὺς πόδας συμβεβηκότα, *diique ipsi non gradiuntur sed fluunt.* Their statues were said to stand rather upon columns than upon legs, for they seem to have been nothing but columns shaped out into this or that figure, the base whereof carrying little of the representation of a foot. These things being premised, I suppose it easy for the intelligent reader to find out the true etymology of *Empusa, quasi* ἐν ποσιν οὖσα, or βάινουσα, from going on her feet, whereas the other *gods* and *demons* had a different gait. If any can dislike this deduction, and think her so named from ἔνιπους, whereas she always went upon two legs, (if her shape permitted it) though she might draw the one after her, as a man doth a wooden leg : I say, if any, notwithstanding what hath been said, can join issue with the Doctor, my reply shall be Σοὶ μὲν ταῦτα δοκοῦντ' ἐϛὶν, ἐμὸι δὲ τάδε.

Now, as to the words of Aristophanes upon which the Scholiast descants, they are these :— speaking of an apparition strangely shaped, sometimes like a camel, sometimes like an ox, a beautiful woman, a dog, &c. Bacchus replies :

Ἔμπουσα τοινὺν γ'ἐϛι.
ΞΛ. πυρὶ γοῦν λάμπεται
ἅπαν το προσωπον, καὶ σκελος χαλκοῦν ἔχει.
ΔΙ. Νὴ τὸν Ποσειδῶ, καὶ βολιτινον θάτερον.
ΞΑ. Σἀφ' ἴσθι.

The Scholiast hereupon tells us that *Empusa,*

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was Φαντασμα δαιμονιῶδες ὑπὸ Ἑκάτης ἐπιπεμπόμενον καὶ φαινόμενον τοῖς δυστυχοῦσιν, ὃ δοκεῖ πολλὰς μορφας αλλασσειν καὶ οἱ μεν φασιν ἀυτην μονοποδα εἶναι, καὶ ἐτυμολογοῦσιν' οιονεὶ ἑνιποδα, διὰ το ἑνὶ ποδι κεχρῆσθαι. And this is all that is material in the Scholiast, except that he adds by and by, that βολιτινον σκελος is all one with the leg of an ass. And this very text and Scholiast is that to which all the authors he names, and more, do refer.

I come now to Stephen, who, in his index, and in the word ποδίζω, gives the derivation of *Empusa*. Ποδιζω, *gradior*, *incedo*, (not to hop) *sic Suidas* Ἔμπουσαν *dictam ait* παρὰ το ἑνὶ ποδιζειν. In the index thus: *sunt qui dictam putent* παρὰ τὸ ἑνὶ ποδίζειν, *quod uno incedat pedi, quasi* Ἔμπουσαν, *alterum enim pedem æneum habet.* But neither Stephen, nor any else, except *Suidas*, whom the hypercritical Doctor had not seen, no, not the Scholiast of Aristophanes (a better critic than Mr. Hobbes) doth relate the etymology as their own. Nay, there is not one that saith *Empusa* hopped on one leg, which is to be proved out of them. The great Etymological Dictionary deriveth it ταρὰ τὸ ἐμποδιζειν, to *hinder*, *let*, &c. its apparition being a token of ill luck. But, as to the Doctor's deduction, it saith, Ἔμπουσα Ψιλοῦπαι, εἰ καὶ δοκεῖ παρὰ τὸ ἕνα συγκεῖσθαι. It doth only *seem* so. And it is strange that ἐν should not alter only its *aspiration*, but change its ν into μ, which I can hardly believe admittable in Greek, least there should be no difference betwixt its derivatives and those of ἐν. When I consider the several μορμόνες which the Grecians had, some whereof did fly, some had no legs, &c, I can think that the origin of this name may have been thus: some amazed person

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saw a *spectrum*, and, giving another notice of it, his companion might answer, it is Βριμὼ, Μορμὼ Ἡκὰτη, but he, meeting with a new phantasm, cries, ἐν ποσὶ βαίνει or βαδίζει, for which apprehension of his, somebody coined this expression of Ἔμπουσα. It may also be possibly deduced from Ἐμποδίζω, so that τύχη ἐμποδιζουσα might afterwards be reduced to the single term of *Empusa*. Nor do I much doubt but that those who are conversant in languages, and know how that several expressions are often jumbled together to make up one word upon such like cases, will think this a probable origination. I believe, then, that Mr. Hobbes's friend did never tell him it was in Eustathius, or that *Empusa* was an *hopping phantasm*. It had two legs and went upon both, as a man may upon a wooden leg. Ἔμπουσα is also a name for Lamia, and such was that which Menippus might have married, which, I suppose, did neither hop nor go upon one leg, for he might have discovered it. But Mr. Hobbes did not except against the derivation, (although he might justly, derivations made afterwards carrying more of fancy than of truth, and the Doctor is not excused for asserting what others barely relate, none approve), but asked him where that is, in what authors *he read that boys' play to be so called.* To which question, the Doctor, to show his reading and the good authors he is conversant in, replies, *in Junius's Nomenclator, Rider and Thomas's Dictionary, sufficient authors in such a business*, which, methinks, no man should say that were near to so copious a library. It is to be remembered that the trial now is in Westminster School, and amongst Ciceronians, neither whereof

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will allow those to be sufficient authors of any Latin word. Alas, they are but *Vocabularies*; and, if they bring no author for their allegation, all that may be allowed them is, that, by way of allusion, our modern play may be called *Ludus Empusæ*. But that it is so called we must expect, till some author do give it the name. These are so good authors, that I have not either of them in my library. But I have taken the pains to consult, first, Rider; I looked in him, (who was only author of the English Dictionary) and I could not find any such thing. It is true, in the Latin Dictionary, which is joined with Rider, but made by Holyoke; (O that the Doctor would but mark!) in the index of obsolete words, there is *Ascoliasmus*, *Ludus Empusæ*, *Fox to thy hole,* for which word, not signification, he quoteth Junius. The same is in Thomasius, who refers to Junius in like manner. But could the Doctor think the word obsolete, when the play is still in fashion? Or, doth he think that this play is so ancient as to have had a name so long ago, that it should now be grown obsolete? As for Junius's interpretation of *Empusa*, it is this: *Empusa, spectrum, quod se infelicibus ingerit, uno pede ingrediens.* Had the Doctor ever read him, he would have quoted him for his derivation of *Empusa*, I suppose. In Ascoliasmus, he saith, *Ascoliasmus, Empusæ Ludus, fit ubi, altero pede in aere librato, unico subsiliunt pede:* ἀσκολιασμὸς *Pollux; Almanicè, Hinckelen; Belgicè, Op een been springhen; Hinckepincken, Flandris.* But what is it in English he doth not tell, although he doth so in other places often. What the Doctor can pick out of the Dutch I know not; but, if that do not justify him, as I think it doth not, he hath wronged Junius, and greatly imposed upon his readers.

But, to illustrate this controversy further, I cannot be persuaded the Doctor ever looked into Junius, for, if he had, I am confident, according to his wonted accurateness, he would have cited Pollux's *Onomasticon* into the bargain, for Junius refers to him, and I shall set down his words, that so the reader may see what *Ascoliasmus* was, and all the Doctor's authors say *Ludus Empusæ* and *Ascoliasmus* were one and the same thing. Julius Pollux (lib. ix. cap. 7): Ὁ δε Ἀσκολὶασμὸς, (old editions read it, Ἀ'σκολιασμὸς et ασκολιάζω) τοῦ ἑτέρȣ ποδὸς ἀιωρȣμένȣ, κατὰ μόνȣ τοῦ ἑτέρȣ πηδᾶν ἐπόιει; ὅπερ Ἀσκωλιάζειν ὠνόμαζον· ἤτοι εἰς μῆκος ἐνήλλαντο, ἢ ὁ μὲν ἐδίωκεν οὕτως, ὁι δὲ ὑπέφευγον ἐπ᾽ ἀμφοῖν θέοντες, ἕως τινὸς τῳ φερομένῳ ποδὶ ὁ διώκων δυνηθῇ τυχεῖν· ἢ καὶ ϛάντες ἐπήδων, ἀριθμοῦντες τὰ πηδήματα· προσέκειτο γὰρ τῷ πλήθει τὸ νικᾶν. Ἀσκωλιάζειν δὲ ἐκαλεῖτο καὶ τὸ ἐπιπηδᾶν ἀσκῷ κενῷ καὶ ὑποπλέω πνευματος, ἠλειμμένω, ἵναπερ ὀλισθάνοιεν περὶ τὴν ἀλοιφὴν. "So that *Ascoliasmus*, and consequently, *Ludus Empusæ*, was a certain sport which consisted in hopping, whether it were by striving who could hop furthest, or whether only one did pursue the rest hopping, and they fled before him on both legs, which game he was to continue till he had caught one of his fellows, or whether it did consist in the boys' striving who could hop longest. Or, lastly, whether it did consist in hopping upon a certain bladder, which, being blown up and well oiled over, was placed upon the ground for them to hop upon, that so the unctuous bladder might slip from under them and give them a fall." And this is all that Pollux holds forth. Now, of all these ways, there is none that hath any resemblance with our *Fox to thy* *hole ;* but the second: and yet, in its description, there is no mention of beating him with gloves, as they do now-a-days, and wherein the play consists as well as in hopping. It might, notwithstanding, be called *Ludus Empusæ*, but not in any sort our *Fox to thy hole ;* so that the Doctor and his authors are out, imposing that upon Junius and Pollux which they never said. And thus much may suffice as to this point. I shall only add out of Meursius's *Ludi Græci*, that *Ascolia* were not *Ludus Empusæ* but *Bacchisacra*, and he quotes Aristophanes's Scholiast in Plutus, 'Ασκώλια ἑορτὴ Διονύσȣ ἀσκὸν γαρ ὄινȣ πληροῦντες, ἐνὶ ποδὶ τοῦτον ἐπεπήδον, καὶ ὁ πηδήσας ἆθλον εἶχε τὸν ὂινον. As also Hesychius, Ασκωλιάζειν, κυρίως τὸ ἐπὶ τοῦς ἀσκοὺς ἅλλεσθαι.

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But I could have told the Doctor where he might have read of *Empusa* as being the name of a certain sport or game, and that is, *in Turnebus Adversaria*, lib. xxvii. cap. 33. There he speaks of several games mentioned by Justinian in his *Code*, at the latter end of the third book, one of which he takes to be named *Empusa ;* adding withal, *that the other are games, it is indisputable,* only *Empusa in lite et causa erit, quod nemo nobis facile assensurus sit Ludum esse, cum constet spectrum quoddam fuisse formas, varie mutans. Sed quid vetat eo nomine Ludum fuisse? Certe ad vestigia vitiatæ Scripturæ quam proximo accedit.* Yet he only is satisfied in this conjecture, till somebody else shall produce a better. And now what shall I say? Was not Turnebus as good a critic, and of as great reading as Dr. Wallis, who had read over Pollux, and yet is afraid that nobody will believe *Empusa* to have been a

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game, and all he allegeth for it is, *quid vetat?* Truly, all I shall say, and so conclude this business, is, that he had read over an infinity of books, yet, had not had the happiness, which the Doctor had, to consult with ***Junius's Nomenclator, Thomasius and Rider's Dictionary, authors sufficient in such a case.***

I now come to the Doctor's last and greatest triumph, at which I cannot but stand in admiration, when I consider he hath not got the victory. Had the Doctor been pleased to have conversed with some of the fifth form in Westminster School, (for he needed not to have troubled the learned master), he might have been better informed than to have exposed himself thus.

Mr. Hobbes had said that ϛιγμὴ signified ***a mark with a hot iron;*** upon which saying the Doctor is pleased to play the droll thus: "Prithee tell me, good Thomas, before we leave this point, (O the wit of a divinity doctor!) who it was told thee that ϛιγμὴ was a mark with an hot iron, for it is a notion I never heard till now, and do not believe it yet. Never believe him again that told thee that lie, for as sure as can be, he did it to abuse thee; ϛιγμὴ signifies a distinctive point in writing, made with a pen or quill, not a mark made with a *hot iron,* such as they brand rogues withal; and, accordingly, ϛίζω διαϛίζω, ***distinguo, interstinguo,*** are often so used. It is also used of a ***mathematical*** point, or somewhat else that is very small, ϛιγμὴ χρόνȣ, a moment, or the like. What should come in your cap, to make you think that ϛῖγμη signifies a mark or brand with a *hot iron?* I perceive where the business lies; it was ϛίγμα ran in your mind when you talked of ϛιγμὴ, and, because the words are somewhat alike, you jumbled them both together, according to your usual care and accurateness, as if they had been the same."

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When I read this I cannot but be astonished at the Doctor's confidence, and applaud him who said, ἀμάθεια ϑάρσὸς φέρει. That the Doctor should never hear that ϛιγμὴ signifies *a mark with a hot iron*, is a manifest argument of his ignorance. But, that he should advise Mr. Hobbes not to believe his own readings, or any man's else that should tell him it did signify any such thing, is a piece of notorious impudence. That ϛτιγμὴ *signifies a distinctive point in writing made with a pen or quill*, (is a pen one thing and a quill another to write with?) nobody denies. But, it must be withal acknowledged it signifies many things else. I know the Doctor is a *good historian*, else he should not presume to object the want of history to another; let him tell us how long ago it is since men have made use of pens or quills in writing; for, if that invention be of no long standing, this signification must also be such, and so it could not be that from any allusion thereunto the mathematicians used it for a point. Another thing I would fain know of this great historian, how long ago ϛίζω and διαϛίζω began to signify *interpungo?* For, if the mathematics were studied before the mystery of printing was found out, (as shall be proved whenever it shall please the Doctor, out of his no reading, to maintain the contrary), then the *mathematical* use thereof should have been named before the *grammatical*. And, if this word be translatitious, and that sciences were the effect of long contemplation,

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the names used wherein are borrowed from talk, Mr. Hobbes did well to say, that ϛιγμὴ precedaneously to that *indivisible* signification which it afterwards had, did signify a *visible mark* made by a hot iron, or the like. And, in this procedure, he did no more than any man would have done, who considers that all our knowledge proceeds from our senses; as also that words do, *primarily*, signify things obvious to *sense*, and only *secondarily*, such as men call *incorporeal*. This leads me to a further consideration of this word. Hesychius, (of whom it is said that he is *Legendus non tanquam Lexicographus, sed tanquam justus author*), interprets ϛιγμὴ, νυγμὴ, which is a point of a greater or lesser size, made with any thing. So ϛίζω signifies to prick or mark with anything in any manner, and hath no impropriated signification in itself, but according to the writer that useth it. Thus, in a *grammarian* ϛίζω signifies to *distinguish*, by *pointing* often; sometimes, even in them, it is the same with ὀβελίζω; sometimes it signifies to set a mark that something is wanting in that place, which marks were called ϛιγμαί. In matters of policy, ϛίζω signifies to *disallow*, because they used to put a ϛιγμὴ (not ϛίγμα) before his name who was either disapproved or to be mulcted. In punishment it signifies to *mark* or *brand*, whereof I cannot at present remember any other ways than that of an *hot iron*, which is most usual in authors, because most practised by the ancients. But, that the mark which the *Turks* and others do imprint without burning may be said ϛίζεσθαι, I do not doubt, no [illegible]lian did to give that term [illegible] whom he says, τὰ σώματα

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ἐϛίζοντο γραφαῖς ποικίλαις, καὶ ζώων παντοδαπῶν ἐικόσι. Thus, horses that were branded with κάππα and σαν (κοππάτιαι and σαμφοραι) were said ϛίζεσθαι. Thus, in its origin, ϛιγμὴ doth signify a *brand or mark with an hot iron*, or the like; and that must be the proper signification of στιγμὴ, which is proper to στίζω, none but such as Dr. Wallis can doubt. In its *descendants* it is no less evident, for, from στιγμὴ comes *stigmosus*, which signifies to be branded; *Vitelliana cicatrice stigmosus*, not *stigmatosus*. So Pliny in his Epistles, as Robert Stephen cites it. And στιγματιας (the derivative of στιγμὴ, which signifies any mark, as well as a brand, even such as remain after stripes, being black and blue), was a nickname imposed upon the grammarian Nicanor, ὅτι περὶ στιγμῶν ἐπολυλόγησε. And, though we had not any examples of στῖγμὴ being used in this sense, yet, from thence, for any man to argue against it, (but he who knows no more than Stephen tells him) is madness, unless he will deny that any word hath lost its right signification, and is used only, by the authors we have, although neither the Doctor nor I have read all them, in its analogical signification. I have always been of opinion, that στιγμὴ signified a *single point*, big or little, it matters not; and στίγμα, a *composure of many;* as γραμμὴ signifies a *line*, and γράμμα a *letter*, made of several lines. For στίγμα signified the *owl*, the *sæmæna*, the letter K, yea, *whole words, lines, epigrams* engraven in men's faces; and στιγμὴ, I doubt not, had signified *a single point*, had such been used, and so it became translatitiously used by grammarians and mathematicians. I could give grounds for this conjec-

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ture, and not be so impertinent as the Doctor in his sermon, where he told men that σοφός was not in Homer; that from ἄφρων came *ebrius;* that *sobrietas* was not bad Latin, and that *sobrius* was once, as I remember, in Tully. Is this to speak suitably to the oracles of God, or rather to lash out into idle words? Hath the Doctor any ground to think these are not impertinences? Or, are we, poor mortals, accountable for such *idle* words as fall from us in private discourses, whilst these ambassadors from heaven *droll* in the pulpit without any danger of an after-reckoning?

But I proceed to a further survey of the Doctor's intolerable ignorance. His charge in the end of the *school-master's* rant is, that he should *remember* στίγμα and στιγμὴ *are not all one.* I complained before that he hath not cited Robert Stephen aright; now I must tell him he hath been negligent in the reading of Henry Stephen: for in him he might have found that στίγμα was sometimes all one with στιγμή, though there be no example in him wherein στιγμή is used for στίγμα. Hath not Hesiod, (as Stephen rightly citeth it), in his *Scutum,* 166-67.

> Στΐγματα δ' ὥς ἐπέφαντο ϊδεῖν δεινοῖσι δράκουσι
> Κυανέα κατὰ νῶτα

ubi scholiastes ὥσπερ δὲ στιγμαὶ ἦσαν ἐπάνω, τῶν ῥάχεων τῶν δρακόντων, κατάστικτοι γὰρ καὶ ποικίλοι ὁι ὄφεις. So Johannes Diaconus upon the place, a man who (if I may use the Doctor's phrase) was *as good a critic as* the Geometry Professor.

Thus much for the *Doctor.* To the understanding *reader,* I say that στιγμὴ is used for burning with a

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hot iron: 2 *Macchab.* ix. 11, where speaking of Antiochus's lamentable death, his body putrefying and breeding worms, he is said, ἐις ετίγνωσιν τοῦ θεοῦ ἔρχεθαι θείᾳ μάστιγι, κατα στιγμὴν ἐπιτεινόμενος ταῖς ἀλγηδόσι; *being pained as if he had been pricked or burned with hot irons.* And that this is the meaning of that elegant writer, shall be made good against the Doctor, when he shall please to defend the vulgar interpretation. Pausanias, in *Bœoticis*, speaking of Epaminondas, who had taken a town belonging to the Sicyonians, called Phœbia (Φουβία) wherein were many Bœotian fugitives, who ought, by law, to have been put to death, saith he dismissed them under other names, giving them only a ***brand*** or ***mark:*** Πόλισμα ἑλὼν Σικυωνὶων Φουβίαν, ἔνθὰ ἦσαν το πολὺ οἱ Βοιώτιοι φυγάδες, στιγμήν ἀφίησι τοῦς ἐγκαταληφθέντας ἄλλην σφίσιν ἣν ετυχε πατρίδα ἐπονομάζων ἑκάστω. It is true στιγμὴν is here put *adverbially*, but that doth not alter the case. Again, Zonaras, in the third tome of his History, in the life of the Emperor Theophilus, saith, that when Theophanes and another monk had reproved the said emperor for demolishing images, he took and *stigmatized* each of them with twelve *iambics* in their faces: εἶτα καὶ τὰς ὄψεις ἀυτῶν κάτεστιξε καὶ ταῖς στιγμαῖς μέλαν ἐπέχεε γράμματα δὲ ἐτύπουν τὰ στιγματα, τὰ δὲ ἦσαν ἴαμβοι οὗτοι. A place so evident, that I know not what the Doctor can reply. This place is just parallel to what the same author saith in the life of Irene, τάς ὄψέις σφών καταστιξας ἐν γράμμασι, μέλανος εγχεομένου τοῖς στίγμασι. If the Doctor object that he is a modern author, he will never be able to render him as inconsiderable as Adrianus Junius's *Nomenclator*, Thomasius and Rider. If any will

An extract of a letter concerning the grammatical part of the controversy, &c.

deny that he writes good Greek, Hieronymus Wolfius will tell them, his only fault is περισσολογια, *redundancy* in words, and not the use of *bad* ones.

Another example of στιγμὴ used in this sense, is in the collections out of Diodorus Siculus, lib. xxxiv. as they are to be found at the end of his works, and as Photius hath transcribed them into his *Bibliotheca*. He saith that the Romans did buy multitudes of servants and employ them in Sicily: Οῖς, ἐκ τῶν σωματοτροφείων ἀγεληδὸν απαχθεῖσιν, ἐυθύς χαρακτῆρα ἐπέβαλλον, καὶ στιγμὰς τοῖς σώμασιν. These are the words but of one author, but ought to pass for the judgment of two, seeing Photius, by inserting them, hath made them his own.

Besides, it is the judgment of a great *master* of the Greek tongue, that *stigmata non tam puncta ipsa quam punctis variatam superficiem Græci vocaverunt.* I need not, I suppose, name him, so great a critic as the Doctor cannot be ignorant of him.

Nor, were στίγματα commonly, but upon extraordinary occasions, imprinted with an hot iron. The letters were first made by incision, then the blood *pressed*, and the place filled up with ink, the composition whereof is to be seen in Aetius. And thus they did use to *matriculate* soldiers also in the hand. Thus, did the Grecian emperor, in the precedent example of Zonaras. And if the Doctor would more, let him repair to Vinetus's comment upon the fifteenth Epigram of Ausonius.

And now I conceive enough hath been said to vindicate Mr. Hobbes, and to show the insufferable ignorance of the puny professor, and unlearned

An extract of a letter concerning the grammatical part of the controversy, &c.

critic. If any more shall be thought necessary, I shall take the pains to collect more examples and authorities, though I confess I had rather spend time otherwise, than in matter of so little moment. As for some other passages in his book, I am no competent judge of *symbolic stenography*. The Doctor (Sir Reverence) might have used a cleanlier expression than that of a *shitten piece*, when he censures Mr. Hobbes's book.

Hitherto the letter.* By which you may see *what came into my (not square) cap to call* στιγμὴ *a mark with a hot iron, and that they who told me* that, did no more tell me a lie than they told you a lie that said the same of στίγμα; and, if στίγμὴ be not right as I use it now, then call these notes not στιγμὰς, but στίγματα. I will not contend with you for a trifle. For, howsoever you call them, you are like to be known by them. Sir, the calling of a divine hath justly taken from you some time that might have been employed in geometry. The study of algebra hath taken from you another part, for algebra and geometry are not all one; and you have cast away much time in practising and trusting to symbolical writings; and for the authors of geometry you have read, you have not examined their demonstrations to the bottom. Therefore, you perhaps may be, but are not yet, a geometrician, much less a good divine. I would you had but so much ethics as to be civil. But you are a notable critic; so fare you well, and consider

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* Written by Henry Stubbe, M.A. of Christ Church, Oxford, who was, according to Anthony a Wood, "the most noted personage of his age that these late times have produced."

Conclusion.

what honour you do, either to the University where you are received for professor, or to the University from whence you came thither, by your geometry; and what honour you do to Emanuel College by your divinity; and what honour you do to the degree of Doctor, with the manner of your language. And take the counsel which you publish out of your encomiast his letter; think me no more worthy of your pains, you see how I have fouled your fingers.

THREE PAPERS

PRESENTED TO THE ROYAL SOCIETY

AGAINST DR. WALLIS.

TOGETHER WITH

CONSIDERATIONS

ON DR. WALLIS'S ANSWER TO THEM,

BY

THOMAS HOBBES,

OF MALMESBURY.

THREE PAPERS

PRESENTED TO THE ROYAL SOCIETY.

TO THE RIGHT HONOURABLE AND OTHERS, THE LEARNED MEMBERS OF THE ROYAL SOCIETY, FOR THE ADVANCEMENT OF SCIENCES.

First Paper.

PRESENTETH *to your consideration, your most humble servant, Thomas Hobbes, (who hath spent much time upon the same subject), two propositions, whereof the one is lately published by Dr. Wallis, a member of your Society, and Professor of Geometry; which if it should be false, and pass for truth, would be a great obstruction in the way to the design you have undertaken. The other is a problem, which, if well demonstrated, will be a considerable advancement of geometry; and though it should prove false, will in no wise be an impediment to the growth of any other part of philosophy.*

DR. WALLIS, DE MOTU, *Cap.* v. *Prop.* 1.	THOMAS HOBBES, ROSET. *Prop.* v.
IF there be understood an infinite row of quantities beginning with 0 or $\frac{1}{0}$, and increasing continually according to the natural order of numbers, 0, 1, 2, 3, &c. or according to the order of their squares, as, 0, 1, 4, 9, &c. or according to the order of their cubes, as, 0, 1, 8, 27, &c. whereof the	*To find a straight line equal to two-fifths of the arc of a quadrant.* I DESCRIBE a square A B C D, and in it a quadrant D A C. Suppose D T be two-fifths of DC, then will the quadrantal arc T V be two-fifths of the arc C A. Again let D R be a mean proportional between DC

First Paper.

last is given; the proportion of the whole, shall be to a row of as many, that are equal to the last, in the first case, as 1 to 2; in the second case, as 1 to 3; in the third case, as 1 to 4, &c.

This proposition is the ground of all his doctrine concerning the centres of gravity of all figures. Wherein may it please you to consider:

First, whether there can be understood an infinite row of quantities, whereof the last can be given. Secondly, whether a finite quantity can be divided into an infinite number of lesser quantities, or a finite quantity can consist of an infinite number of parts, which he buildeth on as received from Cavallieri. Thirdly, whether (which in consequence he maintaineth) there be any quantity greater than infinite. Fourthly, whether there be, as he saith, any finite magnitude of which there is no centre of gravity. Fifthly, whether there be any number infinite. For it is one thing to say, that a quantity may be divided perpetually without end, and another thing to say, that a quantity may be divided into an infinite number of parts. Sixthly, if all this be false, whether that whole book of *Arithmetica Infinitorum*, and that definition which he buildeth on, and supposeth to be the doctrine of Cavallieri, be of any

and D T; then will the quadrantal arc R S be a mean proportional between the arc C A and the arc T V.

Suppose further a right line were given equal to the arc C A, and a quadrantal arc therewith described; then will D C, C A, the arc on C A be continually proportional. Set these proportionals in order by themselves.

D C, C A, arc on C A ∺
D R, R S, arc on R S ∺
D T, T V, arc on T V ∺

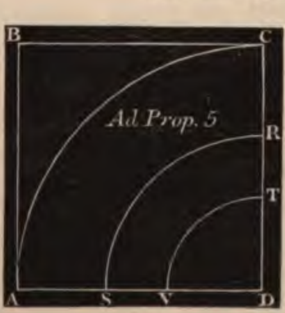
which are in continual proportion of the semi-diameter of the arc. And D C, D R, D T are in a continual proportion by construction, and therefore also C A, R S, T V, and arc on C A, arc on R S, arc on T V, in continual proportion.

Therefore as D C to R S, so is R S to the arc on T V. And D C, R S, the arc on T V will be continually proportional. And because D C, C A, the arc on C A are also continually proportional, and have the first antecedent D C common; the proportion of the arc on C A to the arc on T V is (by Eucl. xiv. 28) duplicate of the proportion of C A to R S, and the arc on R S a mean proportional between the arc on C A and the arc on T V.

Now if D C be greater than R S, also R S must be greater than the arc on T V; and the use for the confirming or confuting of any propounded doctrine.

Humbly praying you would be pleased to declare herein your judgment, the examination thereof being so easy, that there needs no skill either in geometry, or in the Latin tongue, or in the art of logic, but only of the common understanding of mankind to guide your judgment by.

arc C A greater than the arc on R S. Therefore seeing D C, C A, arc on C A, are continually proportional; the arc on T V, the arc on R S, the arc on C A cannot be continually proportional, which is contrary to what has been demonstrated. Therefore D C is not greater than R S. Suppose, then, R S to be greater than D C, then will the arc on R S be a mean proportional between the arc on T V, and a greater arc than that on C A; and so the inconvenience returneth. Therefore the semidiameter D C is equal to the arc R S, and D R equal to T V, that is to say to two-fifths of the arc C A, which was to be demonstrated. Nor needeth there much geometry for examining of this demonstration. Therefore I submit them both to your censure, as also the whole *Rosetum*, a copy whereof I have caused to be delivered to the secretary of your society.



TO THE

RIGHT HONOURABLE AND OTHERS,

THE LEARNED MEMBERS

OF

THE ROYAL SOCIETY,

FOR THE ADVANCEMENT OF THE SCIENCES.

Second Paper

Presenteth to your consideration, your most humble servant Thomas Hobbes, a confutation of a theorem which hath a long time passed for truth; to the great hinderance of Geometry, and also of Natural Philosophy, which thereon dependeth.

THE THEOREM.

The four sides of a square being divided into any number of equal parts, for example into 10*; and straight lines drawn through the opposite points, which will divide the square into* 100 *lesser squares; the received opinion, and which Dr. Wallis commonly useth, is, that the root of those* 100*, namely* 10*, is the side of the whole square.*

THE CONFUTATION.

The root 10 *is a number of those squares, whereof the whole containeth* 100*, whereof one square is an unity; therefore the root* 10*, is* 10 *squares: Therefore the root of* 100 *squares is* 10 *squares, and not the side of any square; because the side of a square is not a superficies, but a*

Confutation of Dr. Wallis's Theorem.

line. For as the root of 100 *unities is* 10 *unities, or of* 100 *soldiers* 10 *soldiers: so the root of* 100 *squares is* 10 *of those squares. Therefore the theorem is false; and more false, when the root is augmented by multiplying it by other greater numbers.*

Hence it followeth, that no proposition can either be demonstrated or confuted from this false theorem. Upon which, and upon the numeration of infinites, is grounded all the geometry which Dr. Wallis hath hitherto published.

And your said servant humbly prayeth to have your judgment hereupon: and that if you find it to be false, you will be pleased to correct the same: and not to suffer so necessary a science as geometry to be stifled, to save the credit of a professor.

TO THE

RIGHT HONOURABLE AND OTHERS,

THE LEARNED MEMBERS

OF

THE ROYAL SOCIETY,

FOR THE ADVANCEMENT OF THE SCIENCES.

Third Paper.

Your most humble servant Thomas Hobbes presenteth, that the quantity of a line calculated by extraction of roots is not to be truly found. And further presenteth to you the invention of a straight line equal to the arc of a circle.

A square root is a number which multiplied into itself produceth a number.

DEFINITION.

And the number so produced is called a square number. For example: Because 10 multiplied by 10 makes 100; the root is 10, and the square number 100.

CONSEQUENT.

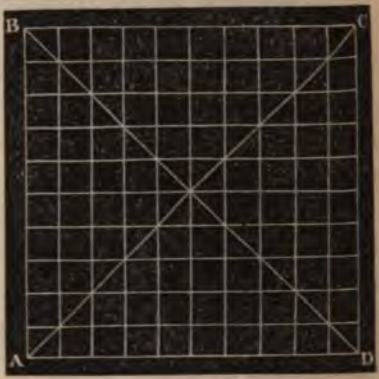
In the natural row of numbers, as 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, &c. every one is the square of some number in the same row. But square numbers (beginning at 1) intermit first two numbers, then four, then six, &c. So that none of the intermitted numbers is a square number, nor has any square root.

PROP. I.

Third Paper.

A square root (speaking of quantity) is not a line, such as Euclid defines, without latitude, but a rectangle.

Suppose A B C D be the square, and A B, B C, C D, D A, be the sides, and every side divided into 10 equal parts, and lines drawn through the opposite points of division; there will then be made 100 lesser squares, which taken altogether are equal to the square A B C D. Therefore the whole square is 100, whereof one square is an unit; therefore 10 units, which is the root, is ten of the lesser squares, and consequently has latitude; and therefore it cannot be the side of a square, which, according to Euclid, is a line without latitude.



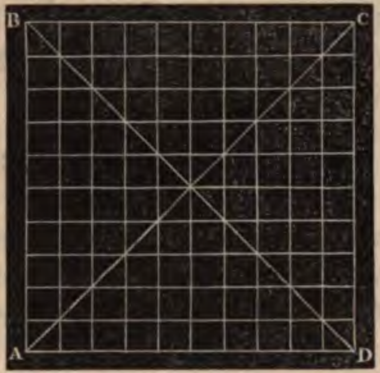
CONSEQUENT.

It follows hence, that whosoever taketh for a principle, that a side of a square is a mere line without latitude, and that the root of a square is such a line (as Dr. Wallis continually does) demonstrates nothing. But if a line be divided into what mber of equal parts soever, so the line have dth allowed it (as all lines must, if they be vn), and the length be to the breadth as num- o an unit; the side and the roof will be all of ength.

PROP. II.

Third Paper.

Any number given is produced by the greatest root multiplied into itself, and into the remaining fraction. Let the number given be two hundred squares, the greatest root is $14\frac{4}{14}$ squares. I say that 200 is equal to the product of 14 into itself, together with 14 multiplied into $\frac{4}{14}$. For 14 multiplied into itself makes 196. And 14 into $\frac{4}{14}$ makes $\frac{56}{14}$ which is equal to 4. And 4 added to 196 maketh 200; as was to be proved. Or take any other number 8, the greatest root is 2; which multiplied into itself is 4, and the remainder $\frac{2}{4}$ multiplied into 2, is 4, and both together 8.



PROP. III.

But the same square calculated geometrically by the like parts, consisteth (by Euclid II. 4) of the same numeral great square 196, and of the two rectangles under the greatest side 14, and the remainder of the side, or (which is all one) of one rectangle under the greatest side, and double the remainder of the side; and further of the square of the less segment; which altogether make 200, and moreover $\frac{1}{49}$ of those 200 squares, as by the operation itself appeareth thus:

The side of the greater segment is $14\frac{4}{14}$
$14\frac{4}{14}$
Which multiplied into itself makes 200.

Third Paper.

The product of 14, the greatest segment, into the two fractions $\frac{4}{14}$, that is, into $\frac{4}{14}$ (or into twice $\frac{2}{14}$) is $\frac{56}{14}$ (that is 4); and that 4 added to 196 makes 200.

Lastly, the product of $\frac{2}{14}$ into $\frac{2}{14}$ or $\frac{1}{7}$ into $\frac{1}{7}$ is $\frac{1}{49}$. And so the same square calculated by roots is less by $\frac{1}{49}$ of one of those two hundred squares, than by the true and geometrical calculation; as was to be demonstrated.

CONSEQUENT.

It is hence manifest, that whosoever calculates the length of an arc or other line by the extraction of roots, must necessarily make it shorter than the truth, unless the square have a true root.

The Radius of a Circle is a Mean Proportion between the Arc of a Quadrant and two-fifths of the same.

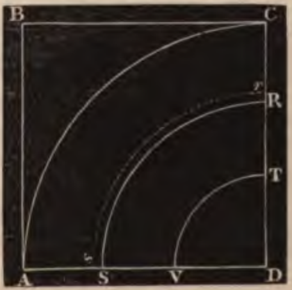
Third paper.

Describe a square A B C D, and in it a quadrant D C A. In the side D C take D T two-fifths of D C, and between D C and D T a mean proportional D R, and describe the quadrantal arcs R S, T V. I say the arc R S is equal to the straight line D C. For seeing the proportion of D C to D T is duplicate of the proportion of D C to D R, it will be also duplicate of the proportion of the arc C A to the arc R S, and likewise duplicate of the proportion of the arc R S to the arc T V.

Suppose some other arc, less or greater than the arc R S, to be equal to D C, as for example *r s*: then the proportion of the arc *r s* to the straight line D T will be duplicate of the proportion of R S to T V, or D R to D T. Which is absurd; because D *r* is by construction greater or less than D R. Therefore the arc R S is equal to the side D C, which was to be demonstrated.

COROL.

Hence it follows that D R is equal to two-fifths of the arc C A. For R S, T V, D T, being continually proportional, and the arc T V being described by D T, the arc R S will be described by a straight line equal to T V. But R S is described by the straight line DR. Therefore D R is equal to T V, that is to two-fifths of C A.



Third paper.

And your said servant most humbly prayeth you to consider, if the demonstration be true and evident, whether the way of objecting against it by square root, used by Dr. Wallis; and whether all his geometry, as being built upon it, and upon his supposition of an infinite number, be not false.

CONSIDERATIONS

UPON THE ANSWER OF DOCTOR WALLIS

TO THE

THREE PAPERS OF MR. HOBBES.

Considerations upon the answer of Dr. Wallis to the three papers of Mr. Hobbes.

DR. WALLIS says, all that is affirmed, is but *if we* SUPPOSE *that, this will follow.*

But it seemeth to me, that if the supposition be impossible, then that which follows will either be false, or at least undemonstrated.

First, this proposition being founded upon his *Arithmetica Infinitorum*, if there he affirm an absolute infiniteness, he must here also be understood to affirm the same. But in his thirty-ninth proposition he saith thus: "*Seeing that the number of terms increasing, the excess above sub-quadruple is perpetually diminished, so at last it becomes less than any proportion that can be assigned; if it proceed in infinitum it must utterly vanish. And therefore if there be propounded an infinite row of quantities in triplicate proportion of quantities arithmetically proportioned (that is, according to the row of cubical numbers) beginning from a point or* 0; *that row shall be to a row of as many, equal to the greater, as* 1 *to* 4." It is therefore manifest that he affirms, that in an infinite row of quantities the last is given; and he knows well enough that this is but a shift.

Secondly, he says, that usually in Euclid, and all after him, by *infinite* is meant but, more than any assignable *finite*, or the greatest possible. I am content it be so interpreted. But then from thence he must demonstrate those his conclusions, which he hath not yet done. And when he shall have done it, not only the conclusions, but also the demonstration, will be the same with mine in Cap. XIV. Art. 2, 3, &c. of my book *De Corpore*. And so he steals what he once condemned. A fine quality.

Thirdly, he says, (by Euclid's tenth proposition, but he tells not of what book), that a line may be bisected, and the halves of it may again be bisected, and so onwards infinitely; and that upon such supposed section infinitely continued, the parts must be supposed infinitely many.

I deny that; for Euclid, if he says a line may be divisible into parts perpetually divisible, he means that all the divisions, and all the parts arising from those divisions, are perpetually finite in number.

Fourthly, he says, that there may be supposed a row of quantities infinitely many, and continually increasing, whereof the last is given.

It is true, a man may say, (if that be supposing) that white is black: but, if *supposing* be *thinking*, he cannot suppose an infinite row of quantities whereof the last is given. And if he say it, he can demonstrate nothing from it.

Fifthly, he says (for one absurdity begets another) *that a superficies or solid may be supposed so constituted as to be* infinitely long, *but* finitely great, *(the breadth continually decreasing in greater proportion than the length increaseth), and so as to have no centre of gravity. Such is*

Toricellio's Solidum Hyperbolicum acutum, and others innumerable, discovered by Dr. Wallis, Monsieur Fermat, and others. But, to determine this, requires more of geometry and logic, (whatsoever it do of the Latin tongue), than Mr. Hobbes is master of.

Considerations upon the answer of Dr. Wallis to the three papers of Mr. Hobbes.

I do not remember this of Toricellio, and I doubt Dr. Wallis does him wrong and Monsieur Fermat too. For, to understand this for sense, it is not required that a man should be a geometrician or a logician, but that he should be mad.

In the next place, he puts to me a question as absurd as his answers are to mine. Let him ask himself, saith he, if he be still of opinion, *that there is no argument in natural philosophy to prove that the world had a beginning*. First, whether, in case it had no beginning, there must not have passed an infinite number of years before Mr. Hobbes was born. Secondly, whether, at this time, there have not passed more, that is, more than that infinite number. Thirdly, whether, in that infinite (or more than infinite) number of years, there have not been a greater number of days and hours, and of which, hitherto, the last is given. Fourthly, whether, if this be an absurdity, we have not then, (contrary to what Mr. Hobbes would persuade us), an argument in nature to prove the world had a beginning.

To this I answer, not willingly, but in service to the truth, that, by the same argument, he might as well prove that God had a beginning. Thus, in case he had not, there must have passed an infinite length of time before Mr. Hobbes was born; but there hath passed at this day more than that infinite length, by eighty-four years. And this day, which is the last, is given. If this be an absurdity, have we not then an argument in nature to prove that God had a beginning? Thus it is when men entangle themselves in a dispute of that which they cannot comprehend. But, perhaps, he looks for a solution of his argument to prove that there is somewhat greater than infinite; which I shall do so far as to show it is not concluding. If from this day backwards to eternity be more than infinite, and from Mr. Hobbes his birth backwards to the same eternity be infinite, then take away from this day backwards to the time of Adam, which is more than from this day to Mr. Hobbes his birth, then that which remains backwards must be less than infinite. All this arguing of infinites is but the ambition of school-boys.

TO THE LATTER PART OF THE FIRST PAPER.

There is no doubt if we give what proportion we will of the radius to the arc, but that the arc upon that arc will have the same proportion. But that is nothing to my demonstration. He knows it, and wrongs the Royal Society in presuming they cannot find the impertinence of it.

My proof is this: that if the arc on T V, and the arc R S, and the straight line C D, be not equal, then the arc on T V, the arc on R S, and the arc on CA, cannot be proportional; which is manifest by supposing in D C a less than the said D C, but
[illegible] another straight line, less than
[illegible] rc on T V; and anybody may
[illegible] f.

[illegible] l by some that think them-

Considerations upon the answer of Dr. Wallis to the three papers of Mr. Hobbes.

selves logicians, why I proceeded upon $\frac{2}{5}$ rather than any other part of the radius. The reason I had for it was, that, long ago, some Arabians had determined, that a straight line, whose square is equal to 10 squares of half the radius, is equal to a quarter of the perimeter; but their demonstrations are lost. From that equality it follows, that the third proportional to the quadrant and radius, must be a mean proportional between the radius and $\frac{2}{5}$ of the same. But, my answer to the logicians was, that, though I took any part of the radius to proceed on, and lighted on the truth by chance, the truth itself would appear by the absurdity arising from the denial of it. And this is it that Aristotle means, where he distinguishes between a direct demonstration and a demonstration leading to an absurdity. Hence it appears that Dr. Wallis's objections to my *Rosetum* are invalid as built upon roots.

TO THE SECOND PAPER.

First, he says that it concerns him no more than other men, which is true. I meant it against the whole herd of them who apply their algebra to geometry. Secondly, he says that a bare number cannot be the side of a square figure. I would know what he means by a bare number. Ten lines may be the side of a square figure. Is there any number so bare, as by it we are not to conceive or consider anything numbered? Or, by 10 nothings understands he bare 10? He struggles in vain, his conscience puzzles him. Thirdly, he says 10 squares is the root of 100 square squares. To which I answer, first, that there is no such figure as a square square. Secondly, that it follows hence, that a root is a superficies, for such is 10 squares. Lastly, he says that, neither the number 10, nor 10 soldiers, is the root of 100 soldiers; because 100 soldiers is not the product of 10 soldiers into 10 soldiers. This last I grant, because nothing but numbers can be multiplied into one another. A soldier cannot be multiplied by a soldier. But no more can a square figure by a square figure, though a square number may. Again, if a captain will place his 100 men in a square form, must he not take the root of 100 to make a rank or file? And are not those 10 men?

TO THE THIRD PAPER.

He objects nothing here, but that *the side of a square is not a superficies, but a line*, and that a *square root (speaking of quantity) is not a line, but a rectangle*, is a contradiction. The reader is to judge of that.

To his scoffings I say no more, but that they may be retorted in the same words, and are therefore childish.

And now I submit the whole to the Royal Society, with confidence that they will never engage themselves in the maintenance of these unintelligible doctrines of Dr. Wallis, that tend to the suppression of the sciences which they endeavour to advance.

LETTERS

AND OTHER PIECES.

LETTERS AND OTHER PIECES.

I.

A LETTER FROM MR. HOBBS TO MY MR.[1]

HONORABLE SIR,

THOUGH I may goe whither and when I will for anie necessity you have of my service, yet there is a necessity of good manners that obliges me as yo^r servant to lett you knowe att all times where to find me. Wee goe out of Paris 3 weekes hence, or sooner, towards Venice, but by what way I knowe not, because the ordinary high way through the territory of Milan is encumbered with the warre betweene the French and the Spaniards. Howsoever, wee have to be there in October next. If you require anie service that I can doe there, it may please you to convey your command by Devonshire house. But if you command me nothing, I have forbidden my letters to look for answer: their busines being only to informe and to lett you knowe that the image of your noblenes decayes not in my memory, but abides fresh to keepe me eternally Your

THO. HOBBS.

Letter I.

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[1] This letter is to be found in the British Museum, amongst the Lansdowne MSS. 238, entitled "a collection of letters to and from persons of eminence in the reigns of Elizabeth, James I, and Charles I, made by some person in the service of Sir Gervas Clifton". It is without date: but the allusion to the war between France and Spain, and the passage in the VITA THO. HOBBES, "Anno sequente qui erat Christi 1629, rogatus a nobilissimo viro domino Gervasio Clifton", &c. (p. xiv), show that it must have been written in either 1629 or 1630.

II.

Letter II.

TO A FRIEND IN ENGLAND.

WORTHY SIR,

I HAVE been behind hand with you a long time for a letter I received of yours at Angers, that place affording nothing wherewith to pay a debt of that kind, all matter of news being sooner known in England than here: and the news you writ me was of that kind, that none from England could be more welcome, because it concerned the honour of Welbeck and Clifton, two houses in which I am very much obliged.

Monsieur having given the slip to the Spaniards at Bruxelles, came to the King about ten days ago at St. Germains, where he was received with great joy. The next day the Cardinal entertained him at Ruelle: and the day after that he went to Limours, where he is now, and from thence he goes away shortly to Bloys, to stay there this winter. The Cardinal of Lyons is going to Rome to treat about the annulling of Monsieur's marriage, which is here by Parliament declared void, but yet they require the sentence of the Pope. There goes somebody thither on the part of his wife, to get the marriage approved: but who that is, I know not. The Swedish party in Germany is in low estate, but the French prepare a great army for those parts, pretending to defend the places which the Swedes have put into the King of France his protection, whereof Philipsbourgh is one; a place of importance for the Lower Palatinate. This is all the French news.

For your question, *why a man remembers less*

Letter II.

his own face, which he sees often in a glass, than the face of a friend that he has not seen of a great time, my opinion in general is, that a man remembers best those faces whereof he has had the greatest impressions, and that the impressions are the greater for the oftener seeing them, and the longer staying upon the sight of them. Now you know men look upon their own faces but for short fits, but upon their friends' faces long time together, whilst they discourse or converse together; so that a man may receive a greater impression from his friend's face in a day, than from his own in a year; and according to this impression, the image will be fresher in his mind. Besides, the sight of one's friend's face two hours together, is of greater force to imprint the image of it, than the same quantity of time by intermissions. For the intermissions do easily deface that which is but lightly imprinted. In general, I think that lasteth longer in the memory which hath been stronglier received by the sense.

This is my opinion of the question you propounded in your letter. Other new truths I have none, at least they appear not new to me. Therefore if this resolution of your first question seems probable, you may propound another, wherein I will endeavour to satisfy you, as also in any thing of any other nature you shall command me, to my utmost power; taking it for an honour to be esteemed by you, as I am in effect,

Your humble and faithful servant

THO. HOBBES.

Paris, Oct.$\frac{21}{31}$, 1634.

My Lord Fielding and his Lady came to Paris on Saturday night last.

III.

Letter III.

TO MY WORTHY FRIEND MR. GLEN.[1]

WORTHY SIR,

I RECEIVED here in Florence, two days since, a letter from you of the 19th of January. It was long by the way; but when it came it did thoroughly recompence that delay. For it was worth all the pacquets I had received a great while together. All that passeth in these parts is equally news, and therefore no news; else I would labour to requite your letter in that point, though in the handsome setting down of it, I should still be your inferior.

I long infinitely to see those books of the Sabbaoth[2], and am of your mind they will put such thoughts into the heads of vulgar people, as will confer little to their good life. For when they see one of the ten commandments to be *jus humanum* merely, (as it must be if the Church can alter it), they will hope also that the other nine may be so too. For every man hitherto did believe that the ten commandments were the moral, that is, the eternal law.

I desire also to see Selden's *Mare Clausum*, having already a great opinion of it.

You may perhaps, by some that go to Paris, send me those of the Sabbaoth, for the other being in Latin, I doubt not to find it in the Rue St. Jaques.

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[1] Probably George Glen, who was installed Prebend of Worcester in 1660, and died in 1669.

[2] The History of the Sabbath. In two books. By Peter Heylyn. 4to. 1636.

Letter III.

We are now come hither from Rome, and hope to be in Paris by the end of June. I thank you for your letter, and desire you to believe that I can never grow strange to one, the goodness of whose acquaintance I have found by so much experience. But I have to write to so many, that I write to you seldomer than I desire; which I pray pardon, and esteem me

Your most affectionate friend
and humble servant
THO. HOBBES.

Florence, Apr. $\frac{6}{16}$ 1636.

My Lord and Mr. Nicholls, and all our company commend them to you.

IV.

LETTER TO SIR CHARLES CAVENDISH.[1]

HONORABLE SIR,

THE last weeke I had the honor to receave two letters from you at once, one of the 30 of Dec., the other of the 7th of Jan., wch I acknowledged, but could not answer in my last. In the first you begin with a difficulty on the principle of Monsr de Cartes, *that it is all one to move a weight two spaces, or the double of that waight one space*, and so on in other proportions: to wch you object the difference of swiftnesse, wch is greater when a waight is moved two spaces then when double waight is moved one space. Certenly de Cartes his meaning was by force the same that mine,

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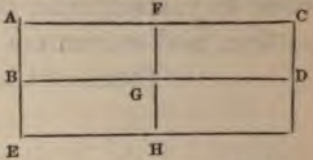
[1] Harleian MS. 6796.

Letter IV.

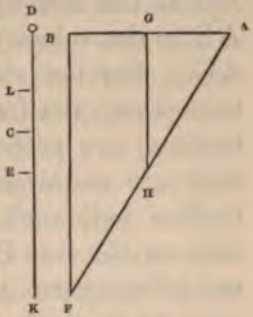
namely, a multiplication of the weight of a body in to the swiftnesse wherewth it is moved. So that when I move a pound two foote a the rate of a mile an howre, I do the same as if of 2 poundes I moved one pound a foote at y^{e} rate of a mile an hower, the other pound another foote at the same rate, not in directũ, but parallell to the first pound. As if the wayt AB were moved to CD at the rate of a mile an howre, 'tis all one as if the waight AE were moved to FH at the same rate. Here is all the difference: this swiftnesse or rate of a mile an howre is, in the first case, layd out in the 2 spaces AG, GC, the latter, in the 2 spaces AG, EG. The first case, as like as if a footman should run wth double swiftnesse endwayes, w^{ch} is y^{e} doubling of swiftnesse in one man: in the other, it is as if you doubled the swiftnesse by doubling the man: for every man has his owne swiftnesse. And so AH is the swiftnesse AG doubled, as well as AD. For that, that Mons^{r} de Cartes will not have just twice the force requisite to move the same weight twice as fast, I can say nothing. The papers I have of his touching that are in my trunk, w^{ch} hath bene taken by Dunkerkers, and taken againe from them by French, and at length recovered by frends I made: but I shall not have it yet this fortnight. In the meane time I am not in that opinion, but do assure myself, the patient being the same, double force in the agent shall worke upon it double effect.



In the same letter you require a better explication of y^{e} proportion I gather betweene wayght

Letter IV.

and swiftnesse: wherein, because you have not my figure, I imagine you have mistaken me very much. And first, you thinke, I suppose, DE equall to AB: w$^{\text{ch}}$ I am sure is a mistake. For I put AB for any line you will to expresse a *minutū secundum.* I will, therefore, go over againe the demonstration I sent you before, and see if I can do it cleerer.



Let AB stand for the time knowne wherein the waight D descendeth to E. And let there bee a cylinder of the same matter the waight D consisteth of, and let the altitude of that cylinder be DC: w$^{\text{ch}}$ I shew before was the swiftnesse wherew$^{\text{th}}$ that cylinder *presseth,* not wherew$^{\text{th}}$ it *falleth.* And wee are now to enquire how farre such matter as the cylinder is made of must descend from D, before it attayne a swiftnesse equall to this pressing swiftnesse DC. And I say it must fall to L. For in the time AB it is knowne that the waight in D will fall to E: and it is demonstrated by Gallileo, that when such waight comes to E, it shall be able to go twice the space it hath fallen in the same time. Therefore the waight D being in E, hath velocity to carry him the space DK (w$^{\text{ch}}$ I put double to DE) in the same time AB. But I put BF equall to DK. Therefore, in the time AB, the waight's velocity acquired in E shall be such as to go from B to F without decrease of velocity by the way. Hence I go on to finde in what point the waight in D comes to where it getteth a velocity equall to

Letter IV.

CD. Therefore, I apply DC to GH, parallel to BF: and then it is, as the time AB to the time AG, so the velocity acquired at the end of the time AB to the velocity acquired at the end of the time AG. For the swiftnesse acquired from time to time (I say, not from place to place, but from time to time) are proportionable to the times wherein they are acquired: w^{ch} is the postulate on w^{ch} Galileo builds all his doctrine. And as AB to AG, so the line BF to the line GH. But, at the end of the time AB, the waight D is by supposition in E, in that degree of velocity as to go BF or DK in the same time AB. The question therefore is, where the waight D shall be at the end of the time AG. For there it hath the velocity of going GH or DC in the same time, because the velocity GH is to the velocity BF as the line GH to the line BF, or as the time of descent AG to the time AB. But, because the spaces of the descent are in double the proportion of the times of descent, make it as BF to GH, that is, DK to DC, so DC to another, DL. The velocity, therefore, acquired in the point of descent E; namely the velocity DK or BF, is to the velocity acquired in the point L, namely, the velocity GH or DC, (w^{ch} is the velocity of the cylinder's waight), as DK to DL. And therefore in L the waight D has acquired a velocity equal to the velocity of the waight of the cylinder.

In the same letter you desire to knowe, how any mediū, as water, retardeth the motion of a stone that falls into it. To w^{ch} I answer out of that you say afterwards, that nothing can hinder motion but contrary motion: that the motion of the water,

Letter IV.

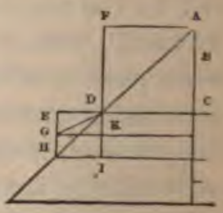
when a stone falls into it, is point blanke contrary to the motion of the stone. For the stone by descent causeth so much water to ascend as the bignesse of the stone comes to. For imagine so much water taken out of the place w^{ch} the stone occupies, and layd upon the superficies of the water: it presseth downeward as the stone does, and maketh the water that is below to rise upwards, and this rising upwards is contrary to the descent, and is no other operation than we see in scales, when of two equal bullets in magnitude that w^{ch} is of heavier metal maketh the other to rise. And thus farre goes your letter of Dec. 30.

For the first quære in your second letter, concerning how we see in the time the lucide body contracts itselfe, I have no other solution but that w^{ch} your selfe hath given: w^{ch} is, that the reciprocation is so quicke, that the effect of the first motion lasteth till the next comes, and longer. For by experience we observe that the end of a firebrand swiftely moved about in circle, maketh a circle of fire: w^{ch} could not be, if the impression made at the beginning of the circulation did not last till the end of it. For if the same firebrand be moved slower, there will appear but a peece of a circle, bigger or lesser according to the swiftnesse or slownesse of y^{e} motion. For the cause of such reciprocation, it is hard to guess what it is. It may well be the reaction of the medium. For though the mediū yeld, yet it resisteth to: for there can be no passion wthout reaction. And if a man could make an hypothesis to salve that contraction of y^{e} sun, yet such is the nature of naturall thinges, as a cause may be againe demanded of such hypothesis: and never should one come to an end wthout as-

Letter IV.

signing the immediate hand of God. Whereas in mathematicall sciences wee come at last to a definition, w^{ch} is a beginning or principle, made true by pact and consent amongst ourselves. Further, you conceave a difficulty how the medium can be continually driven on, if there be such an alternate contraction. To w^{ch}, first I answer, that the motion forward is propagated to the utmost distance in an instant, and the first push is therefore enough, and in another instant is made the returne back in y^{e} like manner. And though it were not done in an instant, yet we see by experience in rivers, as in y^{e} Thames, that the tide goes upward towards London pushed by the water below, and yet at the same instant the water below is going backe to the sea. For seeing it is high-water at Blackwall before 'tis so at Greenwich, the water goes backe from Blackwall when it goes on at Greenwich. And so it would happen, though Blackwall and Greenwich were nerer together then that any quantity given could come betweene.

In my letter from London, speaking of the refraction of a bullet, I thinke I delivered my opinion to be, that a bullet falling out of a thinner medium into a thicker, looseth in the entring nothing but motion perpendicular: but being entred, he looseth proportionably both of one and the other. For suppose a bullet, whose diameter is A B, be in the thiner mediũ, and enter at C into the thicker medium. The thicker medium, at the first touch of B in the point C, worketh nothing upon the line A B. And when the diameter A B is entred, suppose halfe way,



yet the thicker medium operates laterally but on one halfe of it. So that in the somme there is a losse of velocity perpendicular (to the quantity that the diameter AB requires) without any offence to the motion laterall, but so much of the diameter as is within the thicker mediũ is retarded both wayes, and looses of his absolute motion, w^{ch} is compounded of perpendicular and laterall, and that proportionally. Suppose now that a bullet passe from A to D, and receave a peculiar losse of his perpendicular motion by entring at D, so great that he proceed in the perpendicular but halfe so farre, as for example from D to I: and then being in, the thicknesse of the medium take away more of his velocity both perpendicular and laterall, suppose halfe that w^{ch} was left of the perpendicular motion and halfe of his first laterall motion, so that the perpendicular motion is but DK, and the laterall motion DE. Then will the line of refraction be DG. As for that argumentation of Des Cartes, it is, in my opinion, as I have heretofore endeavored to shew you, a mere paralogism.

Lastly, you make this quære, why light hath not at severall inclinations severall swiftnesses as well as a bullet. The bullet itself passeth through the severall media: whereas in the motion of light, the body moved, w^{ch} is the mediũ, entreth not into the other medium, but thrusteth it on: and so the parts of that medium thrust on one another, whereby the laterall motion of the thicker medium hath nothing to worke upon, because nothing enters, but stoppes onely and retardes, in oblique *incidence*, that end w^{ch} comes first to it, and thereby causes a refraction the contrary way to

Letter IV. that of a bullet, in such manner as I set forth to you in one of my letters from hence concerning the cause of refraction. And this is all I can say for the present to the quæres of y^r two last letters.

I have enquired concerning perspectives after the manner of De Cartes. Mydorgius tells me there is none that goes about them, as a thing too hard to do. And I believe it. For here is one Mons^r de Bosne in towne, that dwells at Bloys, an excellent workman, but by profession a lawier, and is counsellor of Bloys, and a better philosopher in my opinion then De Cartes, and not inferior to him in the analytiques. I have his acquaintance by Pere Mersenne. He tells me he hath tryed De Cartes his way, but cannot do it: and now he workes upon a crooked line of his owne invention. He sayes he shall have made one wthin a moneth after he shall returne to Bloys: after that he will see what he can discover in the heavens himselfe, and then if he discover any new thing he will let his way be publique together wth the effects. This is all the hope I can give you yet. So wth my prayers to God to keepe you in prosperity this troublesome time, I rest

Your most humble and obedient servant

TH. HOBBES.

Paris, Feb. 8, *stile no.* 1641.

To the Right Honorable
Sr CHARLES CAVENDYSSHE
present these
/
at Wellinger.

V.

LETTER TO MR. BEALE.[1]

Letter V.

SIR,

THE young woman at Over-Haddon hath been visited by divers persons of this house. My Lord himself hunting the hare one day at the Town's end, with other gentlemen and some of his servants, went to see her on purpose: and they all agree with the relation you say was made to yourself. They further say on their own knowledge, that part of her Belly touches her Back-bone. She began (as her Mother says) to loose her appetite in December last, and had lost it quite in March following: insomuch as that since that time she has not eaten nor drunk any thing at all, but only wetts her lips with a feather dipt in water. They were told also that her gutts (she alwayes keeps her bed) lye out by her at her fundament shrunken. Some of the neighbouring ministers visit her often: others that see her for curiosity give her mony, sixpence or a shilling, which she refuseth, and her mother taketh. But it does not appear they gain by it so much as to breed a suspition of a cheat. The woman is manifestly sick, and 'tis thought she cannot last much longer. Her talk (as the gentlewoman that went from this house told me) is most heavenly. To know the certainty, there bee many things necessary which cannot honestly be pryed into by a man. First, whether her gutts (as 'tis said) lye out. Secondly, whether any excrement

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[1] Amongst the MSS. of the Royal Society.

Letter V.

pass that way, or none at all. For if it pass, though in small quantity, yet it argues food proportionable, which may, being little, bee given her secretly and pass through the shrunken intestine, which may easily be kept clean. Thirdly, whether no urine at all pass: for liquors also nourish as they go. I think it were somewhat inhumane to examin these things too nearly, when it so little concerneth the commonwealth: nor do I know of any law that authoriseth a Justice of peace, or other subject, to restrain the liberty of a sick person so farr as were needful for a discovery of this nature. I cannot therefore deliver any judgment in the case. The examining whether such a thing as this bee a miracle, belongs I think to the Church. Besides, I myself in a sickness have been without all manner of sustenance for more than six weeks together: which is enough to make mee think that six months would not have made it a miracle. Nor do I much wonder that a young woman of clear memory, hourely expecting death, should bee more devout then at other times. 'Twas my own case. That which I wonder at most, is how her piety without instruction should bee so eloquent as 'tis reported.

THO. HOBBES.

Chatsworth, Oct. 20. 68.

VI.

LETTER TO MR. OLDENBURG.[1]

Letter VI.

WORTHY S^{R}

IN the last Transactions for September and October I find a letter addressed to you from D^{r} Wallis, in answer to my LUX MATHEMATICA. I pray you tell me that are my old acquaintance, whether it be (his words and characters supposed to be interpreted) intelligible. I know very well you understand sense both in Latine, Greeke, and many other languages. He shows you no ill consequence in any of my arguments. Whereas I say there is no proportion of *infinite* to *finite*. He answers, he meant *indefinite*; but derives not his conclusion from any other notion than simply *infinite*. I said the root of a square number cannot be the length of the side of a square figure, because a root is part of a square number, but length is no part of a square figure. To which he answers nothing. In like manner, he shuffles off all my other objections, though he know well enough that whatsoever he has written in Geometry (except what he has taken from me and others) dependeth on the truth of my objections. I perceive by many of his former writings that I have reformed him somewhat as to the Principles of Geometry, though he thanke me not. He shuffles and struggles in vaine, he has the hooke in his guills, I will give him line enough: for (which I pray you tell him) I will no more teach him by

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[1] Amongst the MSS. of the Royal Society.

Letter V.

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Letter VI.

replying to any thing he shall hereafter write, whatsoever they shall say that are confident of his Geometry. *Qui volunt decipi, decipiuntur.* He tells you that I bring but *crambe sæpe cocta.* For which I have a just excuse, and all men do the same; they repeat the same words often when they talk with them that cannot heare.

I desire also this reasonable favour from you: that, if hereafter I shall send you any paper tending to the advancement of physiques or mathematiques, and not too long, you will cause it to be printed by him that is printer to the Society, as you have done often for D^r^ Wallis: it will save me some charges.

I am, S^r^,

Your affectionate frend and humble seruant

THOMAS HOBBES.

November the 26*th*, 1672.

ffor my worthy and much honoured
frend M^r^ HENRY OLDENBURGH,
Secretary to the Royal Society.

VII.

No. VII.

TO THE RIGHT HONOURABLE
THE MARQUIS OF NEWCASTLE.[1]

THE passions of man's mind, except onely one, may bee observed all in other living creatures. They have desires of all sorts, love, hatred, feare, hope, anger, pitie, æmulation, and y^e like: onely of curiositie, which is y^e desire to know y^e causes of thinges, I never saw signe in any other living creature but in man. And where it is in man, I find alwaies a defalcation or abatement for it of another passion, which in beastes is commonly prædominant, namely, a ravenous qualitie, which in man is called *avarice*. The desire of knowledge and desire of needlesse riches are incompatible, and destructive one of another. And therefore as in the cognitive faculties reason, so in the motive curiositie, are the markes that part y^e bounds of man's nature from that of beastes. Which makes mee, when I heare a man, upon the discovery of any new and ingenious knowledge or invention, aske gravely, that is to say, scornefully, *what 'tis good for*, meaning what monie it will bring in, (when he knows as little, to one that hath sufficient what that overplus of monie is good for), to esteeme that man not sufficiently removed from brutalitie. Which I thought fit to say by way of anticipation to y^{e} grave scorners of philosophie, and that your lordship, after having performed so noble and honourable acts for defence of your countrie, may thinke it no dishonour in this unfortunate leasure to have employed some thoughts in the speculation of the noblest of the senses, *vision.*

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[1] Harleian MS. 3360: a treatise on Optics, entitled "A minute or first draught of the Optiques. In two parts. By Thomas Hobbes. At Paris, 1646." The second part, *On Vision*, we have in Latin, in the DE HOMINE: the first, *On Illumination*, was never published. The dedication to the Marquis of Newcastle, and the concluding paragraph, is all that is here given of the treatise.

That which I have written of it is grounded especially upon that w^{ch} about 16 yeares since I affirmed to your Lo^{pp} at Welbeck, that light is a fancy in the minde, caused by motion in the braine, which motion againe is caused by the motion of y^{e} parts of such bodies as we call *lucid:* such as are the sunne and y^{e} fixed stars, and such as here on earth is fire. By putting you in mind hereof, I doe indeed call you to witnesse of it: because, the same doctrine having since been published by another, I might bee challenged for building on another man's ground. Yett philosophical ground I take to be of such a nature, that any man may build upon it that will, especially if the owner himselfe will nott. But upon this ground, with the helpe of some other speculations drawne from the nature of motion and action, I have, I thinke, derived y^{e} reason of all the phænomena I have mett with concerning light and vision, both solidly enough nott to be confuted, and withall easilie enough to be understood by such as can give that attention thereto which the figures, whereby such motion as causeth vision is described, do require. All that I shall bee ever able to adde to it, is polishing: for, being the first draught, it could nott bee so perfect as I hope hereafter to make it in Latin.

Butt as it is, it will sufficiently give your Lo^{pp} satisfaction in those *quæres* you were pleased to make concerning this subject. I am content that it passe, in respect of some drosse that yett cleaves to it, for ore: w^{ch} is much better than old ends raked out of the kennell of sophisters' bookes. And for such I commend it to your Lo^{p}, and myselfe to your accustomed good opinion: which hath beene hitherto so greate honour to mee, as I am nott known to the world by any thing so much as by being,

My most noble lord,

Your Lo$^{p's}$ most humble

and most obliged servant

THO. HOBBES.

The treatise ends with the following passage:—

To conclude, I shall doe like those that build a new house where an old one stood before, that is to say, carry away the rubbish.

And first, away goes the old opinion that the *shewes* (which they call visible species) of all objects, are in all places, and all the babble *de extramittendo et intromittendo.* For their species are nothing else but fancie, made by the light proceeding directly or by repurcussion or refraction

No. VII. made from the object to ye eye, and so moving the braine and other parts within.

Secondly, the opinion which Vitellio takes for an axiome and foundation of his *Catoptricques*, that ye place of ye image by reflexion is in the perpendicular drawn from the object to the glasse. For it is false both in plaine glasses and in sphæricall, whether convexe or concave.

Thirdly, the opinion that light is engendred faster in hard bodies, as glasse, than in thin and fluid, as aire.

Fourthly, that objects are seen by *penicilli* that have their common base in the pupills: for ye center of ye eye is in their common base.

Fifthly, the opinion that there bee other visuall lines by which wee see distinctly besides ye optique axis.

Sixthly, the opinion that perspective glasses and amplifying glasses are best made of hyperbolicall figures.

Seventhly, the opinion that light is a bodie, or any other such thing than such light as wee have in dreames.

Eighthly, that ye object appeares greater and lesse in ye same proportion that ye angles have under which they are seene.

Lastly, is to be cast away the conceipt of millions of strings in ye optique nerve, by which the object playes upon the braine, and makes ye soule listen unto it, and other innumerable such trash.

How doe I feare that ye attentive reader will find that which I have delivered concerning ye *Optiques* fitt to bee cast outt as rubbish among the rest. If hee doe, hee will recede from ye authoritie of experience, which confirmeth all I have said. Butt if it bee found true doctrine, (though yett it wanteth polishing), I shall deserve the reputation of having beene y^e^ first to lay the grounds of two sciences; this of *Optiques*, y^e^ most curious, and y^t^ other of *Natural Justice*, which I have done in my booke DE CIVE, y^e^ most profitable of all other.

VIII.

TO THE KING'S MOST EXCELLENT MAJESTY

THE HUMBLE PETITION OF THOMAS HOBBES[1]

SHEWETH, that though your Majesty hath been pleased to take off the restraint of late years laid upon the pensions payable out of your privy purse, yet your Majesty's Officers refuse to pay the pension of your petitioner without your Majesty's express command.

And humbly beseaceth your Majesty, (considering his extreme age, perpetual infirmity, frequent and long sickness, and the aptness of his enemies to take any occasion to report that your petitioner by some ill behaviour hath forfeited your

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[1] Additional MSS. 4292. Brit. Mus.

No. VIII. wonted favour), that you would be pleased to renew your order for the payment of it in such manner as to his great comfort he hath for many years enjoyed it.[a]

And daily prayeth to God Almighty to bless your Majesty with long life, constant health, and happinesse.

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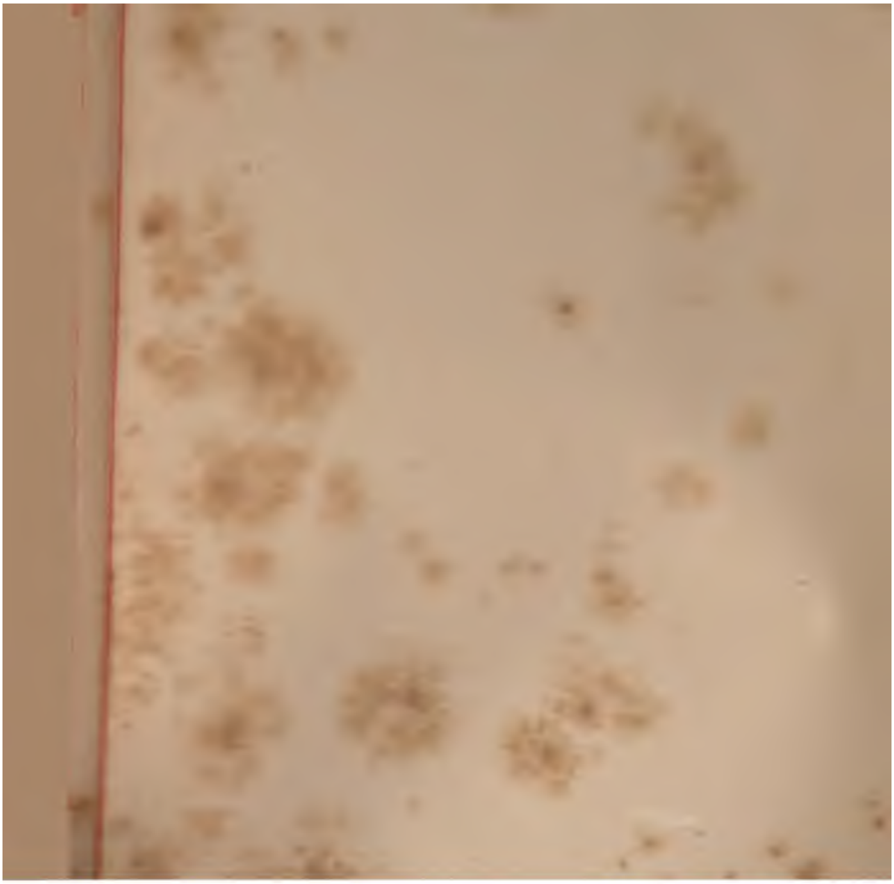
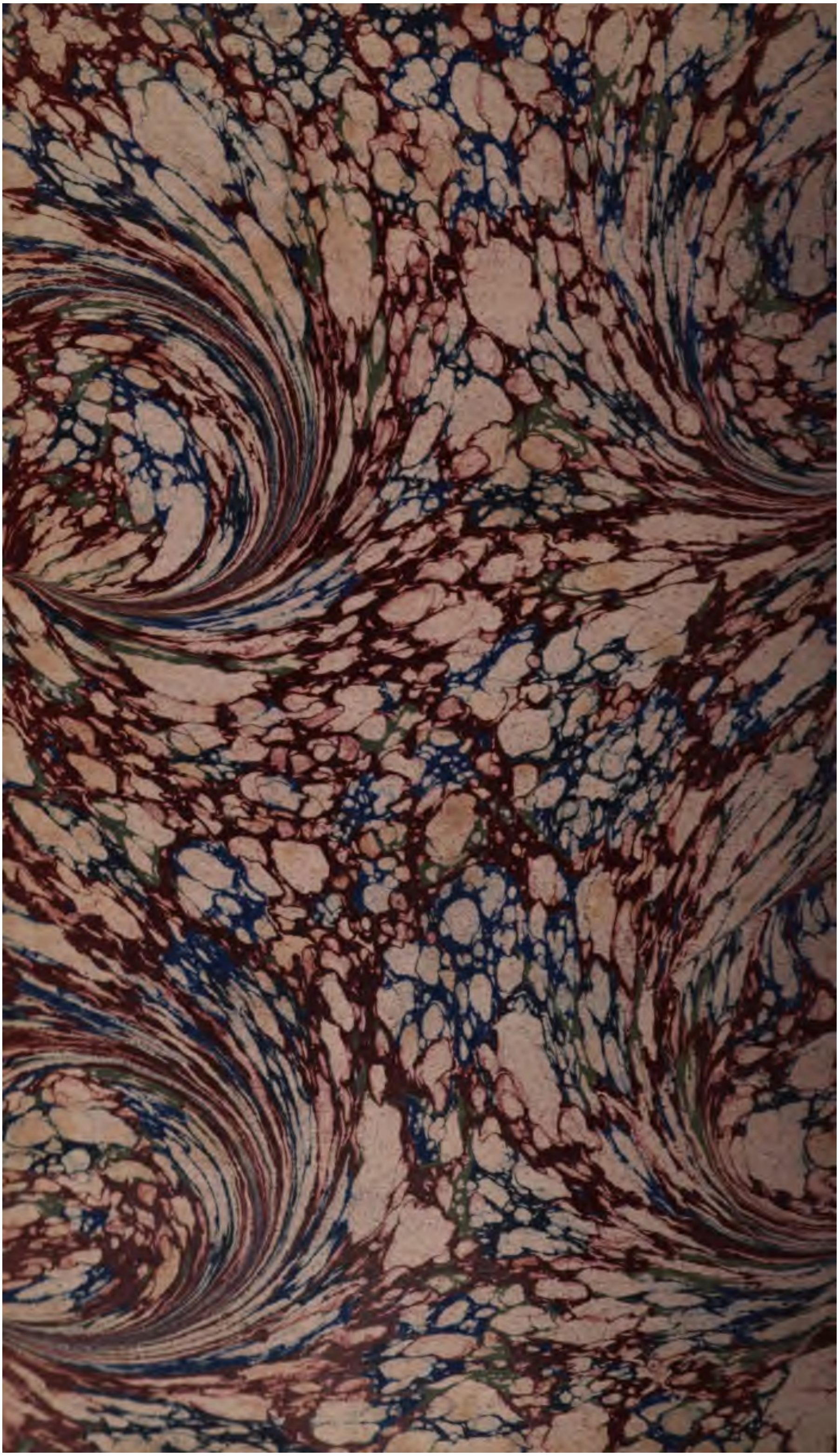
[a] Deinde redux mihi Rex concessit habere quotannis
Centum alias libras ipsius ex loculis:
Dulce mihi donum. Vita *Carm. expres.* p. xcviii.

END OF VOL. VII.

RICHARDS, PRINTER, 100, ST. MARTIN'S LANE.





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